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From the State Institute for Human Genetics, Uppsala, Sweden
Head: Professor Gunnar Dahlberg, M.D., LL.D.

A GENETIC AND NEUROPSYCHIATRIC INVESTIGATION OF A NORTH-SWEDISH POPULATION

with special regard to schizophrenia and mental deficiency

By J. A. BÖÖK

CONTENT

PREFACE

This study is concerned with the genetics and the clinical and statistic features of major neuropsychiatric conditions which occurred during the period of 1902–1949 in a North-Swedish population of approximately 9,000 individuals. It will be published in two parts. The first part deals with the general characteristics of the population and major psychoses. The second part, to be published in a following issue of this journal, deals with mental deficiency and convulsive disorders.

The project was planned in 1946 and the major part of the field work was carried out during 1946 to 1949 when I directed the Department of Medical Genetics at the Institute of Genetics, University of Lund, Sweden. During 1951 to 1953 the work was continued at the Swedish State Institute for Human Genetics, Uppsala, Sweden.

Mrs. *Ruth K. Böök*, M.Sc., permanently worked on the staff as research assistant and genealogist. Her excellent cooperation, competent work and thorough knowledge of the population concerned has been indispensable.

The parishes of Pajala, Junosuando and Muonionalusta were selected for study for two main reasons. They harbour a relatively isolated population with rather pronounced inbreeding. Thus, one could anticipate an increased genetic homogeneity which would further the problems to be analysed. Secondly, part of the time I worked as district physician in the area. Mrs. *Böök* was raised there and knows the population quite well. In a geographically isolated part of the world where "everyone knows everybody" and where the attitude against strangers is characterized by more or less pronounced suspiciousness and reservation, such connections with the population may be of no less substantial value than adequate parish registers or other official data. The collection of the data should also be

seen against this background of personal relationship which secured cooperation not only from all persons holding official positions in the area but also from the entire population to an extent that would otherwise not have been possible.

It was only through the active interest of Professor *Arne Müntzing*, Ph.D., Head of the Institute of Genetics at the University of Lund, that the Department of Medical Genetics was created at his institute in 1946, thus providing a solid background at the outset of this work. For ten years (1939–1949), I had the privilege of belonging to his scientific staff. I take this opportunity to express my profound gratitude and humble obligation to Dr *Müntzing*, an inspiring teacher and friend.

In 1951, Professor *Gunnar Dahlberg*, M.D., LL.D., Head of the Swedish State Institute for Human Genetics at Uppsala, provided the facilities of his institute. Professor *Dahlberg* has followed the analysis of the data in detail during the last two years. It is my pleasure to extend to him my sincere gratitude for many discussions, suggestions, creative criticism and exceptional generosity. Finally, I want to thank Professor *Dahlberg* for making it possible to continue and finish this work parallel with my duties as Assistant Director of the institute.

Under the sponsorship of the State Institute for Human Genetics, Mrs. *Saga Erlander* has been in charge of the statistic calculations. I want to thank her for very conscientious and competent work.

A large number of persons domiciled in the investigation area cooperated during the field work and contributed in various ways. Mr. *Carl M. Alskog* of Junosuando provided many practical arrangements and furthered the project by generous financial support.

The vicars of the three parishes have cooperated far beyond their official duties. Their personal acquaintance with practically each one of their parishioners has been a great help. The Hon. Rev. *Oskar Haapaniemi* of Pajala, *Georg Gripenstad* of Muonionalusta and *Johannes Huhtasaari* of Junosuando did not only supply valuable information but at times engaged themselves personally in the project and interviewed some families which could not be reached by me during the field work. I am happy to acknowledge all of them for the interest, time and effort they devoted to this project.

The medical personnel of the area facilitated the work in every way and generously supplied all available information. I am much indebted to Dr. *John Henriksson*, district physician and director of Pajala Hospital. Among the district nurses who all supplied valuable information and at times engaged themselves personally in the project, I want to mention especially Miss *Hulda Wiman*.

During the course of this work, I have had the opportunity to discuss many clinical and genetic problems with a number of scientists who have contributed valuable suggestions and criticism. Insofar as clinical psychiatry was concerned, I had the privilege to discuss several problems with Professor *Bernhard Jacobowsky*, M.D., Director of the Division of Psychiatry, University of Uppsala. I want to thank him for the interest he has taken in this work. My sincere thanks are also due to Professor *Torsten Sjögren*, M.D., Director of the Division of Psychiatry, Caroline Hospital, Stockholm, who has been kind enough to discuss several pertinent clinical-genetic items with me. *Tage Larsson*, M.D., Ph.D., Director of the Division of Mathematics, Skandia-Freja Insurance Company, Stockholm, has been kind enough to scrutinize and criticize a number of statistical procedures used in this work.

I am very much indebted to the superintendents of several mental hospitals and asylums for facilitating my work by placing files at my disposal and allowing me to examine patients under their care. I want to mention and thank espectally *Nils Sahlström*, M.D., Superintendent of the State Mental Hospital in Piteå, for excellent cooperation.

The cost of this project was in part defrayed by grants from the *Royal Physiographic Society* and the *Reserve Fund for the Advancement of Medical Research*, both at the University of Lund, and from *Kristiane and O. F. Hedström's Memorial Fund*, the *Heijkensköld Fund*, the *Regnell Donation Fund, the Eugenics Society*, all at Uppsala, and from the *Anton and Dorothea Bexelius' Memorial Fund* under the auspices of the Swedish Medical Association.

PART I. PSYCHOSES

CHAPTER I

APPROACH TO THE SPECIFIC CLINICAL AND GENETIC PROBLEMS

INTRODUCTION

In medical genetics, population studies serve two main purposes. The first one is entirely in line with genetic studies of populations in general and aims at an adequate ascertainment of the incidences and morbid risks of different genetic disorders. The second purpose is not so widely recognized. Here the population study is used as a method of clinical and genetic analysis. To be profitable in a clinical-genetic sense, this method can be applied to certain types of populations only. I am referring to so-called isolates (cf. *Wahlund* [1928], *Dahlberg* [1938 and 1947]). One of the main difficulties connected with the twin or family method is diagnostic. Very often too little is known about the relation between conventional clinical entities and their genetic (or supposedly genetic) background. The obvious fact that most clinical entities were created during the pregenetic period and that there is no law saying that these clinical entities correspond to their own specific genetic entities is frequently forgotten. As long as diagnosis in medical genetics is based on the expressions of a superficial phenotype, only, this remains a simple fact never to be forgotten. The common occurrence of similar or identical phenotypic expressions of different genes is well known to the geneticist. There are two ways to explore such a state of affairs, by special cross-breeding experiments or by phenogenetic analysis.

Breeding experiments being out of consideration in as much as human material is concerned and with phenogenetic analysis in its very early infancy, conclusions have to be qualified in this respect. This emerges into the general conclusion that there will be no guarantee whatsoever that *propositi* utilized for twin or family studies, especially if derived from large fluctuating populations, irrespective of their clinical definition, will belong to the same genetic entity. In other words, there is always the possibility that the material is heterogeneous from a genetic viewpoint.

In this very important respect, the study of *propositi* derived from isolates offers certain special advantages. An isolate can be regarded as a structural element of a population of a certain species. It very seldom exists in an absolute sense since the population is made up of a large number of isolates which overlap. The size of the isolate depends primarily on the active or passive mobility of the individual organism. Individuals within an isolate practise more inbreeding than outbreeding which leads to increasing genotypic homogeneity. The isolating mechanism in as much as plants and animals are concerned is very often geographic. Consequently, isolates vary in size and seclusiveness. The smaller the number of individuals and the more effective the isolation, the greater will the increase in genotypic homogeneity tend to become. Another mechanism remains to be mentioned in connection with isolates, namely, genetic drift which has a special significance in relation to genetic diseases in man. Due to the biologic structure of the isolate, genes may be lost or increase to their maximum equilibrium value by mere chance. The probability of such chance fluctuations is greater the smaller the number of individuals of the isolate and the more effective its degree of isolation. The mechanism of fluctuations of gene frequency equilibria in isolates has been analysed by *Wright* from a theoretical viewpoint in a series of papers [1931, 1932, 1940, 1943, 1948]. Changes in equilibria depend on four factors, namely, mutation, selection, migration and genetic drift. It is also important to remember that the size of an isolate refers to its effective size, i.e. the number of actually breeding individuals which is smaller, often considerably smaller, than the number of individuals living at any particular time. The breeding populations of man appear to have been very small throughout history. It is only during recent periods that breakage of isolates has taken place on a larger scale, a process which is now going on at an increasing rate parallel with

technical developments. This does not mean that isolates disappear but rather increase in effective size which is followed by new equilibria tending to become more stable.

The isolate method in medical genetics is applied to relatively small and isolated human populations, i.e. isolates in a more restricted sense. The borders of such isolates may be set by geographic, ethnic or other conditions separating groups of people from unrestricted breeding exchange. Due to the biologic structure of such isolates as outlined above, genetic disorders, originally due to single mutations, may show a considerable variation in frequency in different isolates at any particular time. A specific genetic disorder may be totally absent or have reached its maximum equilibrium in a particular isolate. Furthermore, the total structure of the isolate is relatively more homogeneous genotypically. Consequently, by studying genetic disorders in such isolates we will increase the probability that the clinical entities which we might be able to distinguish also correspond to genetic entities. This gives the isolate method a definite advantage as compared to the twin or family method. It is, however, not a substitute for these other methods. There are a number of drawbacks. There remain probably enough isolates to be studied by the medical geneticist but few offer the same facilities in regard to population statistics and registration as those located in the Scandinavian countries and Switzerland. The number of individuals displaying a specific disorder within the isolate will necessarily tend to be relatively small. What is gained by greater homogeneity will to some extent be lost by smaller series of observed data. This, however, should not obscure the real advantage which consists in the fair chance of obtaining clues of how to differentiate clinical and genetic entities which later on should be tested on a larger scale by the usual twin or family methods.

POPULATION STUDIES IN THE FIELD OF NEUROPSYCHIATRIC GENETICS

General populations.

During the last few decades, a considerable number of studies have been made in this field. The majority have been planned so as to secure adequate average incidence and morbid risk figures for different psychiatric conditions. Apart from the great importance of reliable psychiatric morbidity statistics for the public health services,

the necessity of having such figures for comparison with the results of genetic-statistic analyses of pooled family data was recognized very early. The most important publications in this field have been excellently reviewed and tabulated recently by *Fremming* [1947].

Adequate data have been supplied by the use of one of the following procedures.

1. *The genealogic random test.* The method implies the collection of a random sample of normal *propositi*. The incidence of psychiatric disorders is determined among the relatives of these *propositi*. This method was used strictly only by *Brugger* [1933 a], *Schröder* [1938] and by *Strömgren* [1938], as far as his so-called year group *propositi* are concerned. A number of other writers have selected their *propositi* among people more or less closely connected with hospital populations, e.g. relatives of the spouses of general paretics (*Kattentidt* [1926], *Luxenburger* [1928], *Panse* [1936] and *Dittel* [1936]) or relatives of patients at divisions of internal medicine (*Schulz* [1931], *Bormann* [1937] and others). An objection to the latter type of studies is that the selected populations arrived at do not constitute a true random sample of the general population. However, the errors introduced by using the above-mentioned procedures should be relatively small and of no serious concern.

2. *The birth register test.* This is by far the superior method. Its simple principle is to select from a birth register a suitable number of individuals (either as a random sample or a consecutive series through a certain number of years) who were born some fifty or sixty years ago, details, of course, depending on the type of research and available population registration. These *propositi* are subjected to a thorough follow-up study and the incidences of different psychiatric disorders determined. This method was first used by *Klemperer* [1933]. The value of his study was somewhat reduced as of his 1,000 *propositi* no less than 30 per cent could not be traced. Recently, however, *Fremming* [1947] used the same method starting with 5,500 *propositi*. Adequate information was obtained for about 92.3 per cent of these. *Fremming*'s conscientious study probably contains the most reliable average risk figures obtained so far.

3. *The census method.* The population of a limited geographical area is studied and all individuals who display or have recovered from some psychiatric disorder or other and are living on a certain day are registered. This method has been used among others by *Brugger* [1931, 1933 b and 1938], *Strömgren* [1938] and *Kaila* [1942].

The population size and the effective ascertainment of the psychiatric cases have varied in the different studies.

Details of the technical procedures, computations and errors involved in these methods will be found in the publications of *Strömgren* [1938] and *Fremming* [1947]. These writers have also given comprehensive critical reviews of all important previous studies in this field. Since the publication of *Fremming*'s report in 1947, only two significant studies have come to my knowledge, namely, those of *Sjögren* [1948] and *Schade* [1950]. As mentioned above, the majority of these studies has been undertaken with one single object in mind, i.e. to procure reliable average incidence and risk figures for different psychiatric conditions. The only notable exceptions are the studies of *Strömgren* [1938] and *Sjögren* [1948]. Such studies, therefore, strictly belong to the field of morbidity statistics. Although they are indispensable for genetic psychiatric research in as much as they are concerned with genetic or supposedly genetic diseases, they are not directly concerned with genetic problems. It is true for all of them that they do not attempt to give original contributions to clinical psychiatry. The psychiatric cases have been studied only to justify their classification according to a certain diagnostic scheme. As the studies almost exclusively concern major psychiatry, the diagnostic principles should raise no serious argument. From a clinical genetic standpoint, however, it should be understood that these psychiatric population studies concern the incidences of more or less well-defined clinical phenotypes. To what extent these correspond to specific genetic entities remains an open question. It is granted that in many of these population studies the number of ascertained psychiatric cases was too small to allow a profitable clinical and genetic analysis.

The extensive studies by *Strömgren* [1938] and *Sjögren* [1948] had as one of their main objects the ascertainment of average incidence and morbid risk figures. However, in addition both writers utilized their data for genetic analyses. *Strömgren* [1938] studied the population of the island of Bornholm in the Baltic (population about 46,000). He regarded this population as representative of the average Danish population and it did not have the character of an isolate in the usual sense of that word. *Sjögren*'s [1948] study concerned an island off the Swedish west coast with a population of about 8,700. This was a fairly typical rural and coastal population. The isolation in a demographic sense is only slightly pronounced. It seems

possible, however, that the incidence of first cousin marriages determined at 3.0±0.9 per cent[1] (based on 332 random families) indicates an isolate effect. The average incidence in Sweden at the present time has been estimated at about 0.5 per cent according to *Dahlberg* as given by *Dunn* [1947] and independently by *Romanus* [1953]. It should, however, be observed that these figures were based on questionnaires and therefore probably are too low. *Sjögren* [1949] considers 2–3 per cent to be a correct figure for the Swedish *rural* population. For the City of Stockholm *Romanus* (*l.c.*) found a frequency of cousin marriages of 0.28 per cent whereas some rural provinces (Jämtland, Västerbotten and Norrbotten) displayed a figure of 1.5 per cent. The question whether or not the West-Swedish population should be regarded as an isolate is difficult to decide. In as much as it is a group of people practising more inbreeding than outbreeding, it is an isolate genetically but due to only slightly pronounced geographic isolation, migration and outbreeding probably have had such dimensions that the isolate effect has been relatively small. The same applies to the population of Bornholm where in addition the considerable size of the population reduces the isolate effect. Consequently, the prerequisites are not of such a nature that one has good reasons to assume that genetic diseases occurring in these two populations should show much greater homogeneity than in the total populations of Denmark or Sweden. The results of these studies, which agree fairly well with each other, also support this point of view.

The collection of the psychiatric cases in both studies was made mainly according to the census method as mentioned above. However, in addition to the usual cross section type of study, a longitudinal study was also performed, covering some fifty years prior to the date of the former. Furthermore, *Sjögren* [1948] made a comprehensive genealogic analysis of all his cases.

Isolates.

Population studies of the type just described have been relatively numerous and many of them are of high quality so that by now we possess reasonably accurate incidence and morbid risk figures valid at least for the average central and northwestern European population. On the other hand, there are relatively few studies of isolates in the more restricted sense. Some of these studies concern the incidence

[1] This figure concerns parents of individuals born 1861–1920.

of various genetic diseases in small isolates, numbering some 200–500 individuals, others include genetic analyses (cf. *Mueller* [1933] and *Ruepp* [1935]). Larger isolates were studied by *Sjögren* [1932 and 1935].

It is necessary to separate two ideas in connection with these isolate studies. One is focused on the analysis of the incidences of genetic disorders exclusively. From a more general point of view, it is a study of the geographic (assumed that the borders of the isolate are set by geographic conditions) distribution of genetic disorders. As such, it is one of the methods of analysing the genetic structure of human populations as far as the distribution of disease-producing genes are concerned. The other idea is primarily concerned with the utilization of isolates in regard to their relative genotypic homogeneity and adds up to what was outlined in the introduction as the isolate method of medical genetics.

It is only during relatively recent years that the genetic structure of populations has been subjected to detailed analyses. These achievements (e.g. the concept of the isolate) have only at a slow rate penetrated to clinical geneticists. However, a few of these realized very early the usefulness of examining geographically rather isolated populations which displayed a relatively high degree of inbreeding. The general idea was that recessive conditions would tend to occur with higher frequencies in such regions. *Lundborg* [1913] was one of the first who analysed a special clinical condition (myoclonus epilepsy) within a limited geographic area. He was able to show convincingly that this disease constituted a clinical as well as a genetic entity primarily caused by a single recessive gene difference. In Switzerland, *Hanhart* realized the extremely favourable conditions this country offered the medical geneticist. In 1922 he began his painstaking program of analysing what he calls the inbreeding districts (Inzuchtgebiete) in Switzerland, thus laying the foundations of a genetic nosography (cf. *Hanhart* [1925, 1940, 1941 and 1943]). It is obvious that the population of Switzerland is split up into a large number of isolates of a much more pronounced character than in most other populations. The high consanguinity rate and the relative homogeneity within these isolates and the discontinuous variation between the isolates that one observes is primarily an effect of the structure of this population. This discontinuous variation concerns all kind of genetic traits irrespective of whether they happen to depend on dominant or recessive genes. A

number of studies by *Hanhart* and coworkers gives examples of how dominant as well as recessive genetic diseases and defects have attained rather high equilibria in different Swiss isolates. The relative genotypic homogeneity has also substantially reduced the difficulties of the genetic analysis and important contributions concerning the relation between clinical and genetic entities have been made.

The first one who used the isolate method on a larger scale in psychiatric genetics was *Sjögren* in his studies of 1932 and 1935. With the exception of these two extensive studies, I am aware of no other psychiatric genetic investigations of importance in which the isolate method has been the leading principle.

SCOPE OF THE PRESENT STUDY

The present study is descriptive and analytic. The leading principles have been those mentioned in the introduction and summarized under the name of the isolate method in medical genetics. The purpose has been to examine from a neuropsychiatric and genetic point of view a relatively isolated population which should show appreciable features of being an isolate in a genetic sense. It should be large enough to secure a satisfactory number of cases for clinical, genetic and statistic analysis but not so large as to seriously question a relative genotypic homogeneity in comparison with the population at large. The main object of the study should be to determine the incidence and the clinical and genetic features of major psychoses and oligophrenia (which here includes the low grades, imbecility and idiocy, only). With the project was connected the hope that it would be possible to test the genetic uniformity of previously identified psychotic or oligophrenic syndromes or perhaps to find evidence pointing to the existence of separate clinical and genetic entities within these groups.

It was thought equally important that the selected population should display the smallest possible environmental differences between individuals and families. To that effect, only a rural population could be considered.

For the present study were selected the parishes of Pajala, Junosuando and Muonionalusta in Norrbotten county, Sweden, which in 1949 totalled a population of 8,981. To what extent this population can be considered to satisfy the prerequisites for a practical application of the isolate method as outlined in this paper will be clear from the description to follow.

GENERAL CHARACTER OF THE INVESTIGATION AREA

No special investigations have been made in this region before. It has been mentioned only occasionally in different treatises on Norrbotten county. The following survey is a compilation of data from the publications cited at the end of this section and personal observations. This survey is by no means intended to be exhaustive but rather to give the reader some general understanding of the nature of this region and its population.

Geography. The parishes of Pajala, Junosuando and Munionalasta (together referred to as the investigation area) are located in the extreme north of Sweden some 100 km. north of the Arctic Circle (cf. fig. 1). They occupy a total area of 4,875 sq.km. of which 4,733 sq.km. are land. About 60 per cent of the land is forest, some 2 per cent is cultivated and the rest is moraine- and boglands. The mountainous ground is relatively rich in iron ore which played an important role during the early historic colonisation.

Three rivers flow through the region and connect it with the Baltic (Torne, Muonio and Lainio). Due to a number of rapids and minor falls, none of them can be utilized for regular traffic but they are important for lumber transportation. A great many small lakes are scattered all over the area. The elevation averages some 200 meters above sea level and there are no big mountains, the highest reaching an elevation of approximately 450 meters.

The climate is hard. The mean temperature for the months of January and July are -14^{0}C and $+13^{0}$C respectively. Summer is very short and the number of days with a mean temperature exceeding $+10^{0}$C is only about 70. From the beginning of June to the middle of July the sun never sets ("midnight sun") and from the beginning of December to the middle of January it is constantly below the horizon.

History. Archaeologic discoveries indicate that Norrbotten has been populated at least since the later Stone Age (some 2,500 years B.C.). Stone tools from that age have been found around Pajala. It would seem probable that the people who explored this region during those early days came up from the south. There is then no evidence that the region was populated during the first Iron Age (some 600 years B.C. to 400 A.C.). Later, from the second Iron Age, there is evidence of a scattered population, mostly in the coastal region.

During historic time Swedish colonisation began in the Middle Ages. In the beginning of the fourteenth century the Swedish government started to show some interest in the exploitation of Norrbotten but developments came very slowly. The population of the whole county of Norrbotten in the early sixteenth century has been estimated at 11,000 to 12,000. Some hundred years later it approached 50,000. In fact, not much is known about the investigation area until the middle of the seventeenth century. During 1640 to 1670, iron and copper mills were built in Kengis and Junosuando. Melting houses were operated in Junosuando 1646-1805 and in Tornefors 1710–1873. Pajala village is mentioned since about 1560 as a trading post where Lapps, Norwegians, Swedes, Finns and Russians met. No doubt

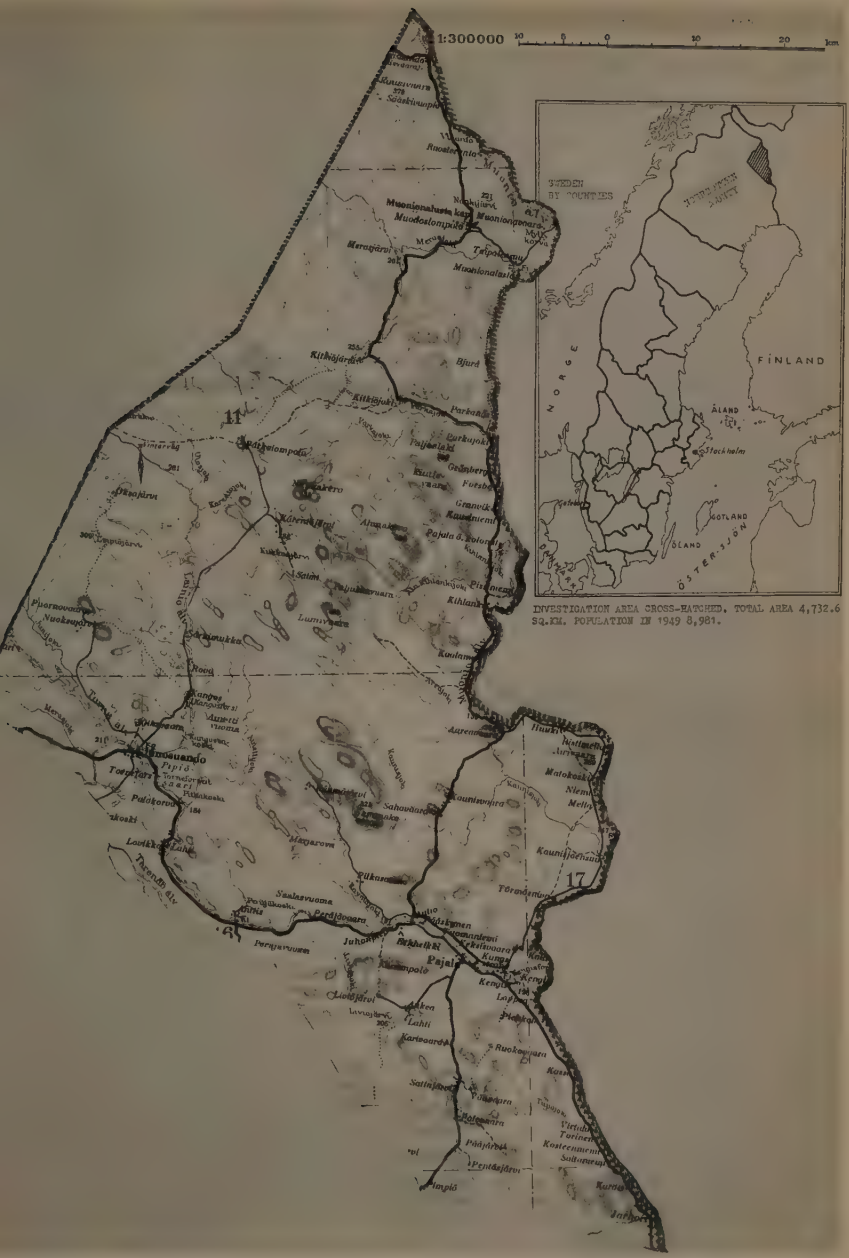


Fig. 1. The parishes of Pajala, Junosuando and Muonionalusta and their location in Sweden.

the late seventeenth and early eighteenth century was an important colonisation period as a considerable number of people from the mining districts in central Sweden moved up here. The parish registers go back to about this time and the family names indicate what has happened. Many families still carry names typic of the mining districts of Dalarna and Värmland. The question of the origin of the present population of the investigation area is difficult to answer satisfactorily as no special study has been made on this topic. The available literature, the study of the different parish records (though not focused on this particular problem) and personal interviews with a number of people living in the region materialized the following tentative explanation. Apart from the immigration of miners just mentioned, other settlers later on came from the south and from adjacent Finnish regions and took up farming and lumbering. Nomadic Lapps came down to the northern parts of the area during the winter and some of these became sedentary and mixed with the rest of the population. However, with the exception of a few small villages in the north, especially Muodoslompolo and Parkalompolo, intermarriages with Lapps or immigration of Lapps do not seem to have taken place to any great extent. The majority of these new settlements seems to have taken place around 1650 to 1730. After 1730 the parish records show relatively insignificant additions of new families. It should be rather safe to conclude that the present population is by and large a result of the propagation of the settlers belonging to the period 1650–1730.

The mining business never became really important. For one thing, transportation was difficult and secondly, the high quality iron ores at Kiruna were discovered in the beginning of the eighteenth century. The mill in Kengis, which was the most important one, was destroyed by the Russians in 1809. The work was taken up again in 1830, only to be discontinued after a short time. In 1879, the whole business was practically finished and has not been taken up again as yet (1953). Since that, the majority of the population has been living on lumbering, modest agriculture and cattle breeding.

Communications. There has probably been something like a road along the coast up to Norrbotten since the sixteenth century. In the inlands, road construction was started late in the seventeenth century. In the early days, most of the traffic occurred on or along the rivers. In 1601, the total sedentary population of Torne Lappmark, which is a vast area, had a population of approximately 500 individuals and in 1605 Pajala village had 8 farms. The settlements in the inlands were very few. In 1718 it took 7 days to travel from the coast to Pajala. In the 1780's a primitive byroad or path led from Torneå on the coast to Övertorneå. In 1850, the rivers were still the most important means of communication. In the same year, the investigation area had less than 20 km. of poor quality byroads. In 1860, a road was built from Övertorneå to Pajala, giving the area its first connection with the Swedish road net. In 1870, this road was extended to Junosuando. Between 1890 and 1920, the Övertorneå road was extended along the river to Areavaara and Kaunisvaara, which settlements in 1890 totalled 196 people. A new better quality road from Pajala to Junosuando was constructed in 1912 and extended to Kiruna in 1914. Muodoslompolo got its first connection with the road net as late as in 1930 and Parkalompolo in 1940. In 1949 there were still a few settlements, totalling some 150 individuals without road connections.

There are still (1953) no railways in the area. The nearest railway station is

located in Övertorneå 116 km from Pajala. The connection from Övertorneå with the Swedish rail net was opened in 1918. Since 1931 a state bus line has been operating between the two last-mentioned villages. Private bus lines operating since about 1930 connect the area with Kiruna and Gällivare (125 km.), both located on the Boden-Narvik line (the iron ore line). Kiruna with 18,385 inhabitants (1949) is the nearest town. The travel by trains and buses from Luleå (county capital with airport and other modern conveniences) in 1949 took 9 ½ hours.

Public health. The first medical service in Norrbotten was created in 1781 when a district physician was stationed in Torneå. In 1850, the county was divided in four medical districts and in 1864, Pajala district was created with a physician located in Pajala village. The first hospital in Norrbotten was built in 1827 in Piteå, followed by Haparanda in 1861, Luleå in 1862, Boden in 1911 and Gällivare in 1913. None of these is closer than about 120 km. from Pajala. In 1932, Pajala medical district was divided in two and at the same time a small secondary hospital, of which one of the district physicians is in charge, was built. This hospital is equipped for all round outpatient service, minor surgery and emergency major surgery. A State mental hospital was opened in Piteå in 1893.

Table 1. Distribution of the working population of the parishes of Pajala, Junosuando and Muonionalusta as to occupation according to the official census of 1945. Total number of individuals 2,818.

	Per cent
Farmers and farm labourers	44.7
Lumbermen and woodcutters	24.9
Factory workers and craftsmen	
1. Miners, stone and metal workers	0.8
2. Wood industries	1.4
3. Food industries	0.7
4. Fabric industries	1.1
5. Leather and rubber industries	1.9
6. Housebuilding	5.4
7. Other	0.3
Transportation services	2.9
Retail business	3.7
Banks and hotels	0.6
Civil servants, professional and semiprofessional workers	
1. Civil service	1.6
2. Public health	2.0
3. Education	3.9
4. Other	0.7
Housework	3.3

General living conditions. The environmental conditions of the investigation area have always been and still are rather uniform. It is a pronounced rural area. With the exceptions mentioned previously, there has been no attempt of any kind at industrialization. The distribution of the working population on different occupations in 1949 is shown in table 1. Approximately 70 per cent is engaged in

either agriculture, cattle breeding or forestry. In as much as the other categories mentioned under "factory workers and craftsmen" are concerned, these should not give the impression of factories or industries as they exist in more densely populated areas of Sweden. Rather, they are small home or family factories which are operated on a modest scale.

The farms are small and most of them can hardly support a family with an average of 5 to 6 children. Most farmers therefore spend part of the year as lumbermen. Up to the 1930's, housing conditions have generally been of poor quality but since that the government housing program for families with three or more children has done much to provide new and better homes. Still many families live under rather primitive conditions, many on similar housing standards as prevailed a hundred or more years ago.

The diet is usually very monotonous. Pork, bacon, cereals, bread and coffee are the most important ingredients. However, a certain improvement has occurred during the last few decades in so far as milk, cheese, margarine and potatoes are used in larger quantities. Green vegetables and fruits, though easily available now, have never gained popularity.

Physical anthropology. Special anthropologic studies in the area are lacking. Table 2 gives some anthropologic data for the population of Norrbotten County taken from the work of *Lundborg* and *Linders* [1926]. The figures, of course, are not strictly representative for the investigation area.

Table 2. Anthropologic observations from Norrbotten County compared with the total of Sweden (*Lundborg* and *Linders*, 1926).

	Norrbotten per cent	Sweden total per cent
Purer Nordic type	19.07	30.82
Light mixed types	24.65	27.12
Purer East Baltic types	15.64	8.68
Medium dark types	27.06	25.13
Dark mixed types	10.80	7.31
Dark types	2.78	0.94
Stature	170.49 ± 0.14 cm	172.23 ± 0.03 cm
Cephalic index	79.19 ± 0.07	77.69 ± 0.01

Lundborg [1923] and *Lundborg* and *Wahlund* [1934] studied the physical anthropology of school children from the northernmost part of Sweden, inclusive of the present investigation area. For Finnish-speaking children (at that time Finnish exclusively was spoken in the homes of the investigation area), they reported 26.8 per cent darkeyed and 73.2 per cent lighteyed in a sample of 2,631. For Swedish-speaking children the corresponding figures were 21.7 and 78.3 per cent. According to *Dahlberg* (personal communication), these figures are not quite correct, the darkeyed being underestimates and the lighteyed overestimates due to inefficient methods of examination. The cephalic indices were for Finnish children 82.3 and for Swedish 80.6. In a study from Finland, *Jalaristo* [1946] found that the cephalic index increases from west to east and north. As evaluated according to

Strömgren's index, the northern parts of Finland displayed a higher incidence of people with pycnic body type. The area bordering Sweden had 51 per cent pycnics. It is, however, not possible to separate pycnics and athletics sufficiently well with the method used here.

Literature on Norrbotten.

Bergfors, G. and *A. Neander* [1928]. Norrbotten. Vol. I and II. Lindblads, Uppsala. – En socialhygienisk undersökning i Västerbottens och Norrbottens län (A study of public health conditions in Västerbotten and Norrbotten counties), performed under the direction of the Royal Medical Board during 1929–31. Vol. I–V. Nordiska bokhandeln, Stockholm 1934. – *Hoppe, G.* [1945]. Vägarna inom Norrbottens län. Studier över den trafikgeografiska utvecklingen från 1500-talet till våra dagar. (The roads in Norrbotten county. A study of communication developments from the sixteenth century to our days). Geographica, publications from the Geographic Institute, University of Uppsala. – *Jalavisto, E.* [1946]. Über die Verteilung der mit dem *Strömgren*schen Index bestimmten *Kretschmer*schen Körperbautypen bei den Finnen. Ann. Acad. Scient. Fenn. Ser. A. 15 : 1–26. – *Lundborg, H.* [1923]. Racial Structure of the Finns of the Northernmost Part of Sweden. Hereditas 4 : 125–132. – *Lundborg, H.* and *F. J. Linders* [1926]. The Racial Characters of the Swedish Nation. Almqvist & Wiksell, Uppsala, G. E. Stechert & Co., London and New York. – *Lundborg, H.* and *S. Wahlund* [1934]. Rassenverhältnisse im nördlichsten Sverige (Schweden). Zeitschr. Morph. u. Anthropol. 34 : 232–243. – Norrbotten, Vol. I and II. Zachrisson Printers, Göteborg 1921. – Norrland, published by the Geographic Society in Stockholm and the Swedish Institute for Industrial Investigations. Industrins Utredningsinstitut, Stockholm, 1942.

COLLECTION OF PRIMARY DATA

The object of this study was a thorough statistic and genetic analysis of major neuropsychiatric disorders which have occurred in the investigation area between January 1, 1902 and September 1, 1949. The first date was selected since the Swedish law of June 14, 1901, concerning the official registration of mentally diseased and mentally defective individuals, came into effect on that day. The latter date was the deadline of registration for this study.

The different official sources which can be utilized for a study like the present one have been described by *Lundborg* [1913] and

Sjögren [1932, 1935 and 1948]. For short but sufficient information, the reader is referred to *Sjögren* [1948] p. 13.

All cases who were registered for this study belong to the field of major neuropsychiatry. The diagnostic criteria will be discussed in detail in a following section. Here it should suffice to say that we are concerned with major psychotic behaviour, oligophrenia in the sense of mental deficiency of such proportions as to make the individual incapable of earning his own living independently, and convulsive disease with grand mal seizures.

The project was started in 1946 with a pilot study to test the cooperation of the population, the conditions of the parish records, the possibilities of reaching the different settlements by car or other vehicles within a reasonable space of time and a number of other practical questions. After that, a detailed program was worked out.

The project was planned as a combined cross sectional, longitudinal and genealogic study. Special attention was paid to the cross section, *i.e.* to a complete ascertainment of all neuropsychiatric subjects who were or had been ill and who were living and resident in the area on September 1, 1949. *Throughout this work, no individual has been taken into account for any other period of time than that spent as living and resident of the investigation area.*

Definition of propositi.

As *propositi* for the period of January 1, 1902 to September 1, 1949 were registered:

1. All cases who had been recorded in the parish registers as mentally diseased, mentally defective or epileptic.

2. All additional cases who had been admitted to the State Mental Hospital in Piteå. I went through all the admission lists at this hospital (the first admissions occurred in 1893) and noted all individuals who were born or at the time of their admission had been domiciled in the investigation area. No other mental hospital takes patients from this area regularly.

3. All additional cases who had been admitted to the Mental Asylum in Råneå and its annexes from 1923. This asylum was first opened in 1903 but all files were destroyed by fire in 1922.

4. The Mental Asylum in Råneå is the first admittance station for mentally deficient individuals from the area. From there they may be sent to a few special asylums in Sweden. Two such special asylums are located in Lund and, as I had an opportunity to visit these, the

admission lists of the Vipeholm Hospital (a State Mental Hospital for uneducable mental defectives who are particularly difficult to handle) and the Institution for the Blind with Complicating Defects were scrutinized. No cases who had not been registered before were detected.

5. All additional cases who had been registered through the Welfare Organization of the State Mental Hospital in Piteå. This organization which has a permanent staff of experienced psychiatrists supervises all neuropsychiatric cases who are cared for in their homes either permanently or while waiting to be admitted or after having left the hospital on trial until finally discharged. This service has been operating since January 1, 1938. The superintendent inspects the area yearly and visits all registered cases and new ones that have been reported to him or otherwise have come to his knowledge. The organization cooperates with different social institutions.

6. All additional cases who had been registered by the district physicians in Pajala. Before 1938 these physicians had also the same duties that were later taken over by the Welfare Organization. Furthermore, they must send a yearly report to the Royal Medical Board. I went through these reports for the period 1917 to 1937. Reports prior to 1917 were not available. As a control measure, the archives of the district physicians in Pajala were scrutinized concerning all files, documents, certificates, etc. to detect information about additional cases. Furthermore, I personally went through every file at the outpatient or lying-in service at Pajala Secondary Hospital to look for additional cases who had received a neuropsychiatric diagnosis.

7. Having performed the procedures mentioned above under points 1 through 6, I considered the registration by official or clinical channels pretty exhausted. There should be left only those individuals who had not contacted a clinic or physician. It had been planned to scrutinize the total living population for additional cases. Now it was, of course, neither practical nor possible to examine some 9,000 people. Instead, the population was screened, i.e. all possible information was collected, noted and checked. At first, the district physicians, the parish clergymen, the school teachers and other persons holding official positions were interviewed as to what they might know or had heard about people who had been or were insane, epileptic or mentally deficient. Furthermore, all district nurses who, on duty, at some time or other had visited at least every home with children

below school age were interviewed. Then I and my assistant visited every village or settlement and interviewed at each place at least one person who on good grounds could be considered as a reliable informant and furthermore had spent at least some 25 or 30 years in the same settlement. Information was also obtained about a number of cases who had died. These were not registered unless the information contained enough details to allow at least a differentiation between psychosis, oligophrenia and convulsive disease.

A *propositus* thus is a neuropsychiatric subject who was registered according to the principles mentioned under points 1 through 7. Some cases, of course, belong to two or more of these categories. Under such circumstances, a case was referred to the category to which he belonged when first ascertained.

Secondary cases.

A secondary case is an affected subject who is ascertained through a *propositus*, i.e. after the attention has been focused on a particular family through the existence of an affected member (= the *propositus*). Due to the exhaustive screening mentioned above, no secondary cases were disclosed in the population living and resident on September 1, 1949. As secondary cases were registered only those family members who were reported as affected at the interviews with the families but had died or migrated prior to my personal contact with the family. None of these secondary cases had been admitted to a hospital. A few siblings of the *propositi* were in this connection reported to have contracted a psychosis after their migration. None of these cases have been included in the data.

The total number of cases ascertained for the period January 1, 1902 to September 1, 1949 was 364. Their distribution on type of registration is shown in table 3.

Further procedures.

For each *propositus* a file and a pedigree chart was laid down. At this point the pedigree charts contained information about personal parish register data (date and place of birth, dates of migrations, date of marriage, date and place of death and domicile) for parents and siblings, only. Further genealogic work was postponed until later. The individual files of the *propositi* in addition for the 261 parish *propositi* contained information if the subject was insane, mentally deficient or epileptic and for the 33 "official" *propositi*

nothing but the diagnosis. The "screening" *propositi* were noted only as being "neuropsychiatric cases for further investigation". They numbered at this time about 65. Since some of them at the personal examination turned out to be neither psychotic, epileptic nor oligophrenic in the sense mentioned previously, the number was finally reduced to 52. It was decided that for the genetic analysis only those families (= parents and siblings) should be accounted for which contained at least one living *propositus*. An exception from this rule was made only in regard to a relatively rare special type of genetic oligophrenia which will be dealt with in part II of this work (genetic spastic oligophrenia). The reason for this was first that the clinical grouping of the *propositi* should as far as possible be based on a personal analysis of these individuals and secondly that those families in which the *propositi* were deceased would not be available for the same intense investigation. The information which could be obtained would be less reliable and more scanty the further back towards 1902 one went. In regard to a clinical and genetic analysis, it is extremely important that the ascertainment of the family data should be as uniform as possible.

Table 3. Crude survey of all neuropsychiatric cases registered in this study. Type of registration as *propositi* or secondary cases.

First registration through:	No. of *propositi*	No. of. secondary cases
1. Parish records	261	
2. Admission to mental hospital	14	
3. Admission to mental asylum	5	
4. Welfare organization	7	
5. District physicians' official reports	7	
6. The writer's screening procedure and family studies	52	17
	347	17
Total no. of cases	364	

Field work.

For the clinical and genetic field work thus remained:

1. All *propositi* who were living and domiciled in the investigation area or, although still residents of the area, were inmates of different institutions or hospitals.

2. Parents and siblings of these *propositi* provided that these relatives were living and domiciled in the area.

All these individuals should as far as was practically possible be subjected to personal interviews and examinations. The extent to which it was possible to examine these individuals personally will be mentioned in connection with the presentation of the genetic analyses of the different clinical conditions.

To approach these *propositi* and their families as unbiased as possible, I did not study any hospital files or other sources of information that were available at this stage. In as much as information about these families had arrived, it was filed for later study. The idea was to secure a first hand independent evaluation of each case.

The domicile of each individual to be examined within the area was marked on a map. A detailed travel plan was agreed upon. From our headquarters which was alternatively Pajala, Junosuando or Muodoslompolo, most places could be reached in one or two hours by car. In most instances, my assistant, Mrs. *Ruth K. Böök*, M. Sc., went to the respective place one or two days in advance to try to prepare the people and get them together. The major part of the field work within the area was carried out during the summers of 1947, 1948 and 1949.

Those *propositi* who were hospitalized were examined in the respective institutions during the fall of 1949. Only a few mentally deficient individuals who were inmates of institutions in Bohuslän and Dalarna could for financial reasons not be examined. However, all had previously been inmates of the Mental Asylum in Råneå and files were available.

The information obtained at the personal interviews and examinations was completed from a number of other so-called objective sources. To that effect my assistant studied the files of the different social aid and protection organizations in Pajala, the Temperance Board and the Child Protection Board. Finally, she interviewed in each settlement a number of people who were no close relatives of the *propositi* families. Most of these informants were school teachers, nurses, midwives or public servants. The greater part of this information has not been utilized for this paper. It was collected only to secure that no individual with a major neuropsychiatric condition would have slipped through the net. As mentioned earlier, the project was not planned to include cases with minor psychic ailments or deficiencies.

The personal examinations have been carried out according to the following principles. At first it was determined whether or not the subject should be regarded as being a neuropsychiatric case in the sense used throughout this study.

Those individuals who apparently did not qualify as belonging to this category were handled separately. They were referred to the category of "unaffected" on the following grounds:

1. At the interview they did not appear psychotic or oligophrenic. This interview was only carried to a point when this differentiation could be made with reasonable security. Any questionable cases were referred to the category of "affected" for further analysis.

2. Previous psychotic episodes or seizures were denied.

3. No information was given by other family members or by other individuals who had been interviewed to indicate that the subject was oligophrenic or had had psychotic episodes or seizures.

4. The subject had not been registered as a *propositus*.

An individual who satisfied all these criteria and thus classified as "unaffected" was subjected to no further examination. Quite a few of these individuals certainly had neuropsychiatric manifestations of various kinds but not of the type and severity that had been determined to be the object of this study. Although I am perfectly aware that a detailed study of these individuals would have been important and very profitable, the execution of such a program was practically impossible under prevalent circumstances.

Remaining subjects who thus had been classified as "affected" or "probably affected" were scrutinized more closely.

1. A complete history was taken. This was for nearly all cases supplemented by information from "unaffected" close relatives or other informants. Naturally, a great number of the "affected" subjects were not able to supply any coherent information about themselves.

2. A physical examination including a neurologic study was performed. It was not considered practical to use standardized forms, as one would not have known in advance what would crop up. No studies were made with special equipment except that the phenylpyruvic test was made on oligophrenic subjects when urine samples could be obtained.

3. A psychiatric analysis was performed. Details of this exami-

nation will be apparent through a study of the symptomatology of the different clinical conditions to be reported under their respective headings.

The "unaffected" family members were interviewed about those parents and siblings who had died or migrated prior to our visit. They were also asked to supply what information they might have about other relatives known to have been insane, mentally deficient or epileptic.

Collection of hospital data.

When all personal interviews and examinations had been performed, the files of all "affected" individuals who at some time or other had been admitted to different hospitals or institutions were photographed on microfilm.

Final clinical files of this study.

When all information had been collected, I analysed the raw data in detail and prepared for each "affected" a file card containing all relevant clinical and personal data. The diagnoses based on my personal examinations were compared with the hospital diagnoses and a final diagnosis agreed upon. Consequently, all cases have been classified according to uniform principles.

Genealogic work.

The directions for this work were simply to trace the ancestors of each *propositus* as far back as possible. This extensive job was done very conscientiously and completely independent of my program by Mrs. *Ruth K. Böök*, M. Sc. During my clinical analyses, I thus had no knowledge of what relationship might exist between the families of the different *propositi*. The usual sources of information available in Sweden were used (see *Lundborg* [1913]). For most of the *propositi*, it was possible to trace their ancestors back to the beginning of the eighteenth century. A total of 10,341 ancestors were investigated during the course of this work which was finished in 1952.

Data for fertility analyses.

A fertility analysis of the *propositi* was adopted as a special part of this project. Complete data on the childbirths, inclusive of stillbirths, of all *propositi* who were living and residents on September 1, 1949 were secured from the parish register. To reduce necessary corrections as much as possible, control data were collected thus:

For each *propositus*, two control individuals (one male and one female) who were born during the same year *and* who were still living and resident in the area on September 1, 1949 were selected as a random sample from the parish registers, the two mentioned criteria being the only principles of selection. For these control individuals, data were collected on marriages and childbirths.

DEMOGRAPHY 1900–1949

The general demographic features which were mentioned previously in the paper will be supplemented by a more detailed treatise of some important items of the population statistics concerning the period covered by this study.

Birth rates.

In figure 2, the crude birth rates per 1,000 for Sweden, Norrbotten county and the investigation area are shown. The rates for the latter population have been distinctly higher during the whole period. It is interesting to note that the population under study did not join in the general decrease that characterized the Swedish population during the 1930's. During the last few years, there is indication of a decrease which probably is related to increased standards of living within the area. The crude birth rates could, of course, be misleading if one disregards the age distributions of the

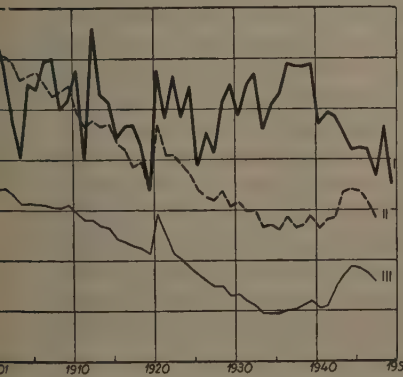


Fig. 2. Crude birth rates per 1,000 for the investigation area (I), Norrbotten county (II) and Sweden (III).

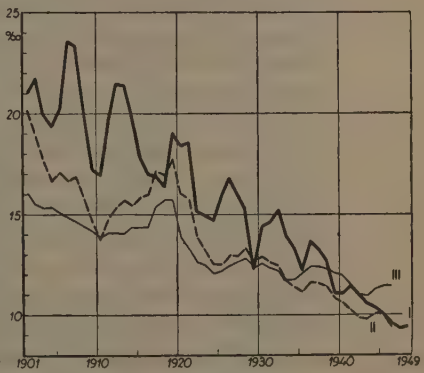


Fig. 3. Crude death rates per 1,000 for the investigation area (I), Norrbotten county (II) and Sweden (III).

populations thus compared. For the populations of Sweden, Norrbotten and the investigation area the percentages below 15 years of age are about 20, 30 and 40 respectively and above 65 there are about 10, 7 and 5 per cent. Consequently, if one considers the birth rates of the breeding populations, the differences which appear in figure 2 are rather underestimated. In table 4, the same three populations have been compared in regard to specific birth rates, i.e. the number of live births per 1,000 females between 15 and 45 years of age. This comparison reflects more adequately the relative fertility. It should be safe to conclude that the population of the investigation area displayed a fertility which was roughly twice that of the average Swedish rural population.

Table 4. Number of live births per 1,000 females aged 15–45 years.

Year	Total Swedish rural population	Norrbotten rural population	Investigation area
1900	13.44	19.84	21.97
1910	12.35	17.31	19.62
1935	6.78	10.50	19.03
1945	7.65	12.66	16.95

Mortality.

The crude death rates per 1,000 for the rural populations of Sweden, Norrbotten and the investigation area are given in figure 3. The general indication of this diagram of a higher but during recent decades decreasing mortality has been substantiated by a more detailed analysis by *Dahlberg* [1952]. Crude death rates, of course, are rather inadequate for comparisons. *Dahlberg*'s analysis concerns the year 1945. The following paragraph was compiled from his paper.

The cumulative death risk for males and females up to the age of 25 years is highest in the northern part of Sweden. For Norrbotten county this risk in 1945 was for males 112.0 per thousand and for females 90.7 and thereby highest in the country which averaged 74.5 and 57.5 respectively. Between 25 and 50 years of age, the same risks were 106.3 and 86.4 against the averages of 80.2 and 69.2. Finally, between 50 and 70 years of age we find the figures of 375.6 and 295.9 against the averages of 320.6 and 274.0.

The higher death rates of the northern parts of Sweden depend on several factors e.g. climate, less availability of medical attention, poverty, hard work. They may also be due to isolate effect (cf.

Dahlberg [1952]). The higher death rates for the younger age groups are above all due to high infant mortality and for the young adults tuberculosis is an important cause of death.

Population size.

High fertility and relative isolation has caused a rapid increase of the population of the investigation area. As is shown in table 5, this increase has been close to 100 per cent during the last fifty years. The age distribution of the population is shown in table 6. Note the relatively large number of individuals in the younger age classes characteristic of an increasing population.

Table 5. Population size of the investigation area during 1900–1949 (per December 31).

Year	Males	Females	Total
1900	2,390	2,194	4,584
1910	2,547	2,339	4,866
1920	2,744	2,604	5,348
1930	3,233	2,964	6,197
1940	4,241	3,962	8,003
1945	4,628	4,039	8,667
1949	4,772	4,209	8,981

Table 6. Survey of the age distribution of the population of the investigation area during 1900–1949.

Age groups	Average population 1900–1945		Population of 1949	
	Males	Females	Males[1]	Females[1]
0 – 4	497	472	664	670
5 – 9	429	422	672	635
10 – 14	397	367	576	519
15 – 19	310	287	403	372
20 – 24	282	235	414	287
25 – 29	242	193	350	262
30 – 34	196	175	309	306
35 – 39	194	151	307	233
40 – 44	158	130	244	182
45 – 49	140	119	204	148
50 – 54	119	106	153	152
55 – 59	103	99	137	129
60 – 64	90	68	113	86
65 – w	158	149	245	209
Totals	3,315	2,973	4,791	4,190

[1] Figures calculated on the basis of the 1945 official census.

Migration.

To get reliable figures on migration which would have immediate relevance to genetic interpretations would be an extremely laborious task. It would mean a detailed study of each individual who had moved in and out of the three parishes during the last fifty years. Migrations reported in the official statistics are rather undifferentiated. There is no way to separate between a migration 5 km. to an adjacent parish and to a parish say in south Sweden. From a genetic viewpoint, the significance is quite different. Nor is it possible to find out how many of those who immigrate were actually born in the area, which is especially common in regard to the young women who have worked only a couple of years in the cities and then return to marry and raise a family at home. Still, a comparison of crude migrations should be valuable as it reflects rather closely the general mobility of the population. In figure 4, the migrations of the investigation area have been compared with those of the isolates studied by *Sjögren* [1932 and 1935] and the cities of Luleå, Piteå, Boden and Haparanda in Norrbotten county. It is apparent that the mobility of the population is of the same order of magnitude as in the isolates studied by *Sjögren*. It is, of course, no surprise that the mobility of the city populations is so much more pronounced. The curves have

Fig. 4. Migration rates per 1,000. I and II in- respectively outmigrations for the investigation area. III and IV in- respectively outmigrations for the North-Swedish isolates investigated by *Sjögren* [1932 and 1935]. V and VI in- and outmigrations for the cities of Norrbotten county.

been drawn to show the relative difference, only, in comparison with populations of what one might call ordinary mobility.

From this survey and the data presented therein, it would seem justified to summarize that the high fertility, the rapid increase of the population and its relative immobility, especially as concerns immigration, in addition to what has been described previously in regard to the geography of the area and so forth, complete a fair picture of an isolate as this term is used in population genetics.

SURVEY OF THE TOTAL MATERIAL

The 364 neuropsychiatric cases mentioned in table 3 (p. 21) have been divided into three major groups, namely 1. a psychotic group, 2. an oligophrenic group and 3. a group of convulsive disorders. These groups will be dealt with in detail separately. Table 7 and 8 give a first general orientation of the neuropsychiatric material.

Table 7. Survey of all discovered psychiatric cases who at the time of their illness have been living and resident in the area but have died or moved before Sept. 1, 1949. Period of 1902–1949.

Diagnosis	Number of individuals Males	Females	Total
Psychotic group	(48)	(29)	(77)
Schizophrenia	22	16	38[1]
Schizophrenia?	17	8	25
Manic-depressive	0	2	2
Involutional	2	1	3
Senile	1	0	1
Other psychoses (unknown type)	6	2	8
Oligophrenic group	(26)	(33)	(59)
Idiocy or imbecility of unknown etiology	17	27	44[1]
Spastic oligophrenia	6	5	11
Mongolism	2	1	3
Idiocy or imbecility of probable exogeneous origin	1	0	1
Group of convulsive disorders (Grand mal)	(8)	(15)	(23)
Convulsive disease without idiocy or imbecility	4	9	13
Convulsive disease with idiocy or imbecility	3	2	5[2]
Convulsive disease with spastic oligophrenia	0	1	1[3]
Convulsive disease with schizophrenia	1	3	4[4]

[1] inclusive of 2 ♀♀ with the diagnosis schizophrenia + oligophrenia.
[2] included in the group "idiocy or imbecility of unknown etiology" above.
[3] included in the group "spastic oligophrenia" above.
[4] included in the group "schizophrenia" above.

The total number of individuals studied is 147.

Table 8. Survey of all discovered psychiatric cases living and resident Sept. 1, 1949, inclusive of those who had recovered. Population 8,981.

Diagnosis	Number of individuals		Total
	Males	Females	
Psychotic group	(62)	(44)	(106)
Schizophrenia	50	35	85[1]
Schizophrenia?	6	2	8
Manic-depressive	0	2	2[2]
Involutional	2	1	3[2,3]
Senile	1	0	1
Encephalopathy	2	0	2
Other psychoses	1	4	5
Oligophrenic group	(58)	(41)	(99)
Idiocy or imbecility of unknown etiology	40	28	68[1,3]
Spastic oligophrenia	9	4	13
Mongolism	4	6	10
Idiocy or imbecility of probable exogeneous origin	5	3	8
Group of convulsive disorders (Grand mal)	(21)	(14)	(35)
Convulsive disease without idiocy or imbecility	13	8	21
Convulsive disease with idiocy or imbecility	5	5	10[4]
Convulsive disease with spastic oligophrenia	1	0	1[5]
Convulsive disease with schizophrenia	2	1	3[6]

[1] inclusive of 4 ♂♂ and 4 ♀♀ with the diagnosis schizophrenia + oligophrenia.
[2] inclusive of 1 ♀ with the diagnosis of manic-depressive psychosis + involutional psychosis.
[3] inclusive of 1 ♂ with the diagnosis of involutional psychosis + oligophrenia.
[4] included in the group "Idiocy or imbecility of unknown etiology" above.
[5] included in the group "spastic oligophrenia" above.
[6] included in the group "schizophrenia" above.

The total number of individuals studied is 217.

Concerning the representativity of the present data, not much needs to be said as this is fairly evident from what was reported under "Collection of primary data". Cases who have died or moved prior to September 1, 1949 to some extent represent a selection in regard to severity. It is granted that an additional unknown number who have not come to the attention of some official authority might have existed during 1902–1946. Especially a number of cases who have not troubled their families too much must have passed unnoticed. The most important contribution by performing this longitudinal registration has been to secure the ascertainment of all cases alive at the cross-section date inclusive of those who had recovered or at any rate were not in institutions. Furthermore, those for whom adequate files were available have contributed to the clinical analyses.

For reasons given previously in this paper, they have, however, not been included as *propositi* in the genetic analyses.

The cross-section cases of table 8 due to the type of their registration constitute an unconditioned representative series.

Genealogy.

The genealogy of this and similar studies contains two major points of information. The first is an objective ascertainment of consanguinities between parents of affected individuals. This item will be dealt with in connection with the specific clinical groups. The second one is a measure of the degree of relationship between all the families of registered cases. This relationship will give an idea of the extent of the genetic homogeneity of the data.

Table 9. Survey of the total number of ancestors ascertained per generation in the present study compared with the isolate studies of *Sjögren* 1932 and 1935. II = parents, III = grandparents and so forth.

Generation	Number of total possible ancestors and percentage ascertained				
	Present data Oligophrenia and psychoses		*Sjögren* [1932]	*Sjögren* [1935]	
			Oligophrenia	Oligophrenia	Psychoses
II	no.	570	80	150	108
	%	96.5	100.0	99.3	96.3
III	no.	1,140	160	300	216
	%	80.5	95.6	91.0	80.1
IV	no.	2,280	320	600	440
	%	63.4	74.7	75.2	56.6
V	no.	4,560	640	1,200	880
	%	45.9	44.8	48.3	34.1
VI	no.	9,120	1,280	2,400	1,760
	%	29.3	16.7	27.1	15.4
VII	no.	18,240	2,560	4,800	3,520
	%	10.9	2.9	13.2	6.7
VIII	no.	36,480	5,120	9,600	7,040
	%	1.6	0.4	4.9	1.2
IX	no.	72.960	10,240	19,200	14,080
	%	0.1	0.1	1.0	0.1
No. of families		285	40	75	54
Total no. of ascertained ancestors		10,341	1,037	3,397	1,424

The 364 cases of tables 7 and 8 belonged to 285 sibships (full siblings). A total of 10,341 ancestors of these sibships have been investigated. The extent of the genealogical ascertainment is shown in table 9 where the data have been compared with *Sjögren*'s genealogies of 1932 and 1935. The results appear very similar in regard to completeness and show what on the average is practically possible to achieve in Swedish rural populations.

Of the total of 285 sibships, it has been possible to join 240 into one large pedigree complex which goes back to 31 ancestral pairs who were living about 1700–1750. This is a direct indication of the relative genotypic homogeneity of the material, making it suitable for analysis according to the isolate method. It should be observed that the genealogic work was performed to arrive at an approximate idea of the genotypic homogeneity of the neuropsychiatric material. It is quite conceivable that the rest of the population would show about the same degree of homogeneity. The result, therefore, does not by itself prove anything about the genetic background of the psychoses or oligophrenias. It does say, however, that the material is relatively homogeneous from a genetic viewpoint and much more so than if one had selected the *propositi* according to the usual family method.

DIAGNOSTIC CLASSIFICATION

Any diagnostic system in neuropsychiatry would be objectionable from some viewpoint or other. Apart from conditions with visible neuropathology, positive neurology or serology, the diagnoses are based on psychologic signs and symptoms which often are difficult to define. In regard to the major psychiatric conditions which will be dealt with in this paper, there are by and large fair agreements about the classification of cases with pronounced symptoms.

Concerning the etiology, the different schools of psychiatry have entirely different ideas which naturally to some extent influence the classification of especially borderline cases. Until the etiological hypotheses have been supported by data which will be generally accepted, a certain amount of subjectivity cannot be avoided. From a practical clinical point of view, it is necessary to have a diagnostic system and some compromise is justified. From a scientific viewpoint, however, it would probably rather be of negative value to adhere too strictly to some particular diagnostic concepts as elaborated by a certain school of thought.

For this study it was considered best to base the original classification on the simplest possible unambiguous signs. Thus one would obtain relatively well-defined groups which later could be subjected to more detailed symptomatologic analyses.

The diagnostic classification used in this study is entirely descriptive. Stress will be laid on a presentation of the criteria in such a way that they should be easily understandable and expressed in generally accepted psychiatric nomenclature. Opinions will necessarily differ in regard to what criteria should be required for a certain diagnosis and which one is more important than the other. In general, the criteria used in this study correspond to those currently utilized in Scandinavian and continental European psychiatry.

Schizophrenia.

I do not think that as yet any important facts have been disclosed which warrant a differentiation between schizophrenia and dementia praecox as suggested by *Bellak* [1948]. Schizophrenia will be used as a descriptive term implying a certain type of maladjustment. It has not been anticipated that schizophrenia should be a clinical entity but a certain type of psychotic behaviour that in regard to its etiology may or may not be uniform. For the diagnosis of schizophrenia, all of the following main symptoms were required:

1. An unmistakable change of personality should be demonstrable.
2. Dissolution of associations and/or a bizarre, incongruous, unintelligible mode of thinking.
3. Emotional disturbances consisting of lack of or decreased affective modulation and faulty emotional contact in personal relations.
4. Autism consisting in withdrawal or otherwise lack of adequate contact with the environment.
5. Acute, subacute or gradual onset before the age of 50.

The following signs have been evaluated as *contributory symptoms:*

C. 1. Hallucinations of a somatic nature, of taste, smell and hearing (definite voices, "Gedankenlautwerden").
C. 2. A feeling of subjection.
C. 3. Ideas of reference.
C. 4. Typical paranoid delusions.

For the different *subgroups* the following criteria were accepted:

I. *Catatonia.* Physical expression of negativism or positivism with one or more of the following components: i. Catalepsy, ii. Stupor, iii. Hyperkinesia, iv. Stereotypy, v. Negativism, vi. "Befehls-automatie", vii. Automatism, viii. Impulsivity.

II. *Simplex type.* Gradual onset. Dominance of main symptoms, especially emotional regression.

III. *Hebephrenia.* Onset before 25 years of age. General dissolution of personality. Dominance of contributory symptoms especially delusions and hallucinations concerned with omnipotence and omniscience.

IV. *Paranoid type.* Dominance of delusions and hallucinations of persecution. Generally well kept personality. Onset after 25 years of age.

V. *Undetermined type.* To this group were referred patients who could not easily be referred to one of the above subgroups due to difficulties in classification or insufficient information about details of symptomatology.

Schizophrenia?

By this diagnosis is meant a psychosis which did not satisfy completely the requirements above as to warrant a *conclusive* diagnosis of schizophrenia but containing obvious schizophrenic components. In some cases, this classification was necessary due to insufficient information about the symptomatology and course of the disease (patient was dead, had migrated or could for other reasons not be examined). In other cases, a conclusive diagnosis could not be reached after personal examination in spite of existing psychosis or due to the fact that the psychotic episode was passed years earlier in a recovered person who had a poor memory of details.

Since later, when all psychotic cases had been analysed, it was disclosed that manic-depressive psychosis was practically absent in the investigation area, the idea that all cases diagnosed as schizophrenia? actually were conclusive schizophrenics was strengthened. Depressive states would not have been referred to this group but in some cases it would have been impossible to differentiate between periodic or chronic manic states and schizophrenia.

Manic-depressive psychosis.

This diagnosis was reserved for acute, periodic and self-limited depressions or manic states or alternations of both occurring before

the age of 50. These symptoms were expected to occur in an emotionally normal individual and not to be of a reactive type. Depressions in neurotic personalities were excluded. Patients with delusions other than affective synthetic, hallucinations and dissociations were not accepted under this diagnosis.

Involutional psychosis.

Under this diagnosis have been included cases with typical restless agitation and depression with or without paranoid features, rigid compulsive obstinacy, obsessional and delusional symptoms. Age of onset 51–69 years.

Senile psychosis.

Delusional or depressive psychoses occurring after the age of 70.

Psychosis during pregnancy or puerperium.

As such were diagnosed short self-limited psychotic episodes of confusional character occurring during pregnancy or puerperium and which could not be referred to the schizophrenic or manic-depressive groups.

Reactive psychosis.

A psychosis which could be explained as a psychologic reaction to an obvious environmental cause.

Alcoholic psychosis.

A psychosis which could not be referred to any other group and occurring in an individual who over a relatively long period of time had been a chronic and excessive alcohol addict.

Oligophrenia.

The diagnosis of oligophrenia was based mainly on a clinical and social evaluation of the case. Thus it means an individual who, due to arrested or incomplete development of mind existing before the age of eighteen years, was incapable of normal social adjustment. This latter incapacity was taken in the sense that the individual could not, due to his mental defect, independently earn his own living or, if he was a child, could not reasonably be expected to do so. Furthermore, the oligophrenics had not been able to attend ordinary public schools. It would be more correct to say that under normal conditions they

would not have been admitted. Due to rather primitive school conditions in the area up to relatively recent years, a number of the oligophrenics of this study were actually admitted in spite of the fact that they turned out to be completely uneducable. The group comprises idiots with an IQ of approximately 0–35 who are uneducable even in special schools and imbeciles with an IQ of approximately 35–70 who to some extent can be educated in special schools or colonies for the feebleminded. I did not perform any psychometric tests. However, most of the institutionalized oligophrenics had been tested. Apart from the general unreliability of such tests, they would have been of no great value for the present study. What was attempted was to sort out those individuals who show the sharp clear-cut difference, the pathologic variants. For the purpose of studying whether specific entities could be disclosed among the oligophrenics of this population, the dividing line against the higher-grade defectives who might be considered as constituting the tail of the normal intelligence curve need only be approximately correct. As there is considerable overlapping concerning the IQ's of such entities, psychometric tests would not have made the differentiation easier or more adequate than the combined clinical and social evaluation that was relied upon here.

The different specific entities belonging to this heterogeneous oligophrenic group will be dealt with in part II of this work.

Convulsive disorders.

Individuals with this diagnosis comprised only those who had or had had seizures of typical grand mal character. Some type of personality change was not required for a diagnosis.

CHAPTER II

SCHIZOPHRENIA

INTRODUCTORY REMARKS

The concept of schizophrenia as used in this study should not deviate appreciably from the current views of the majority of neuropsychiatrists. Insofar as there are divergent opinions, the present concept should rather be too restricted. My concept, concordant with that of the majority of Scandinavian psychiatrists,

originates mostly from the views of *Kraepelin* and *Bleuler*. Concerning the symptomatic classification, these views do not deviate appreciably from the concepts of *Freud*, *Jung*, *Meyer* and their followers as to whom should be regarded as a schizophrenic subject. In regard to the possible etiology there are, of course, hardly any agreements at all. Thus, I think that the cases of this study who have been diagnosed as schizophrenics would in all quarters be recognized as such. I have not attempted to include in the present schizophrenic group so-called borderline cases of which several types have been described, e.g. the pseudoneurotic form of schizophrenia (*Hoch* and *Polatin* [1949]), confusional and diluted forms and so forth.

The diagnostic classification used in this study served the only purpose of securing all psychotic subjects about whom there should be no reasonable doubt that they belonged to the schizophrenic group. In the following sections, these individuals will be subjected to a clinical and genetic analysis. As the character of the investigation area in many respects is unique, it was deemed most adequate as far as possible to utilize primary data taken from the studied population for the different calculations even if this procedure sometimes had the consequence that the observed series became relatively small from a statistic point of view.

SYMPTOMATOLOGY

In tables 7 and 8 there are 123 cases with a conclusive diagnosis of schizophrenia. Of these 120 have been utilized for an analysis of the clinical features, 3 cases were omitted since they migrated relatively soon after the onset of the disease and most of the information about their symptomatology referred to a period when they were no longer residents of the area. 85 cases were living and resident on September 1, 1949 and of these 76 have been examined by me.

Most of these 120 cases have been under observation for a considerable period of time, as is shown in table 10.

In accordance with the criteria mentioned previously, these cases have been referred to the following subgroups:

	Males	Females	Total
Catatonia	65	43	108
Simplex	1	1	2
Undetermined	5	5	10

Table 10. Time from onset of the disease to death or to the end of the observation period (September 1, 1949).

No. of years observed	0	½	1	2	3	4	10	15	20 and over	Total
M		1	1	2	1	6	11	12	9	28	71
F							7	9	10	23	49
Totals		1	1	2	1	6	18	21	19	51	120

Catatonia should not, of course, be taken too literally, i.e. as indicating a distinct or pure group, but in the sense that there was a preponderance of catatonic signs. Some individuals should perhaps be regarded as catatonic-hebephrenic borderline cases.

Premorbid personality.

For 102 cases it was possible to obtain a positive statement whether or not abnormal characterologic traits had been observed prior to the onset of the disease. As such abnormalities were reckoned abnormal shyness and preoccupation, a tendency to violent outbreaks without adequate motivation, suspiciousness, rigid stubbornness, religious fanaticism, emotional inflexibility, despotic tendencies, alcoholism and minor criminal acts. Most of the traits that have been reckoned as abnormal should no doubt be called schizoid or schizothymic according to the terminology of *Kretschmer*. With reservation for all the subjectivity that is necessarily involved in such an evaluation, the results are given in table 11.

The result justifies no other conclusion than that *at least* some 50 per cent of the cases had shown some traits which were experienced as abnormal by the relatives.

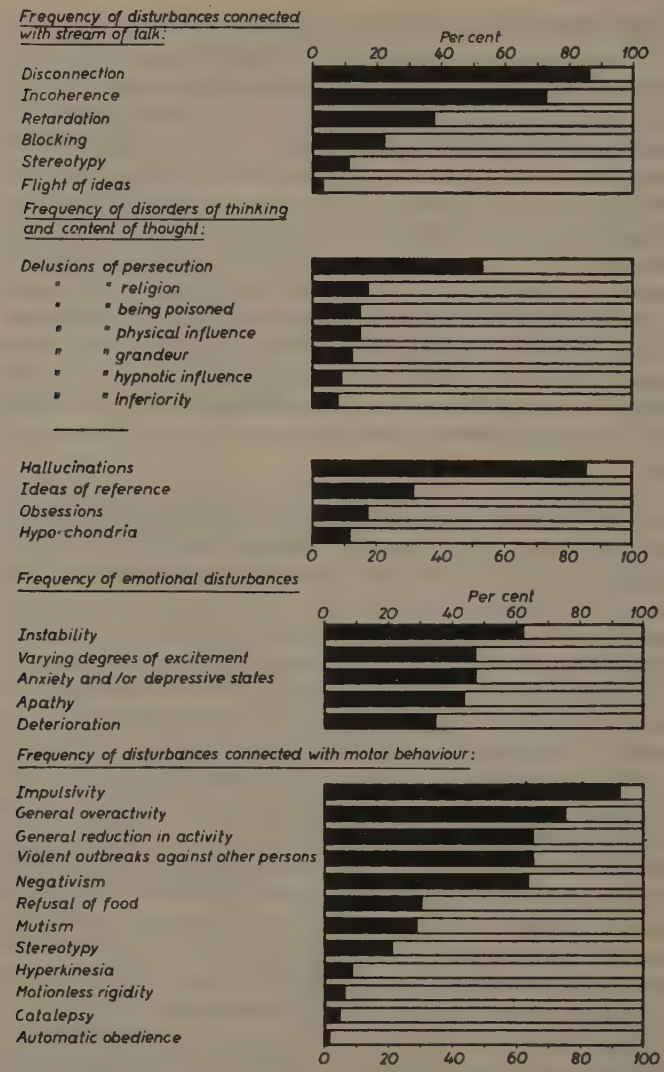
Table 11. Schizophrenia. Survey of premorbid abnormal personality traits.

Deviation	No. of cases	Percentage (120)	Percentage (102)
Reported as normal by relatives	40	33.3	39.2
Intellectual inferiority	9	7.5	8.8
Abnormal personality	53	44.2	52.0
No information	18	15.0	—

Clinical features of the schizophrenic psychoses.

To avoid any lengthy description, the main symptoms which were observed have been tabulated diagrammatically in figures 5 and 6. These diagrams give the percentage of each registered symptom and refer to the whole period of observation (cf. table 10) which necessarily differs from case to case. They show which symptoms have been present at one time or another. I did not intend to study in detail the different phases of the disease and, besides, the number of cases is too small to allow a sufficient number of subdivisions for such a purpose. Rather the aim was to obtain a fair picture of the average symptomatology. On a purely statistic basis, each case represents an observation time of roughly 18 years and the diagrams give an approximate idea of the symptoms prevalent for such a period. One must, of course, make reservations for different kinds of incompleteness and inconsistencies which are inherent in any histories and files of this sort.

There remains no doubt that the catatonic signs are outstanding. Thus the conclusion mentioned previously that about 90 per cent of the cases belong to the catatonic subgroup which was based on a clinical evaluation of each case separately received further support through the joint analysis of the different signs. But it does not only seem justified to refer the majority of cases to the catatonic subgroup, in many respects they display rather far-reaching similarities of catatonic symptoms and course of the disease. These similarities would seem to justify an attempt at a general description of the schizophrenic of this area as all features cannot very well be expressed in tables and figures. The onset is usually insidious over a period of a few months to several years. The patient looses interest in his work, avoids people and becomes increasingly apathetic and preoccupied. He does not seem to care about his family, wants to be left alone, is easily irritated and arguments with the family are common. Later outbreaks of sudden violence are extremely common. The patient threatens family members or neighbours with murder or assault in connection with rather unsystematized delusions of persecution. During such periods of aggressiveness and impulsivity, it often comes to actual assault or attempted suicide which then is the usual cause of hospitalization. During this time there is, of course, also a development of the combination of schizophrenic basic symptoms. The subsequent course in about 75 per cent of the cases was characterized by a definite periodicity in as much as periods of



Figs 5 and 6. Diagrammatic representation of the symptomatology of 120 individuals with a conclusive diagnosis of schizophrenia. On a purely statistic basis each case represents an observation time of about 18 years and the diagrams show what symptoms have occurred at one time or another.

general reduction of activity and more or less complete withdrawal changed with periods of overactivity, impulsivity, aggressiveness and excitement. Sudden episodes of violence were very common. Between such periods or episodes, the patient may recover more or less completely and become able to go back to work. In a few cases, the patients recover, at least socially, after one or several attacks and stay relatively well. A great many, however, proceed towards extreme deterioration. Most of those who "recover" show residual defects of primarily psychomotor or affective nature.

Body type.

A rather subjective evaluation of the typology according to *Kretschmer's* ideas made by me (76 cases) or taken from hospital files (24 cases) resulted in this subdivision; leptosomes 37 per cent, athletics 46 per cent and pycnics 17 per cent.

The body type was also classified on the basis of *Rohrer's* index calculated for 85 cases (table 12).

Table 12. Schizophrenia. Body type by *Rohrer's* index.

	Leptosome		Athletic	Pycnic	
	— 1.10 —	1.30 —	1.40 —	1.50 —	Total
Males . . .	6	20	12	7	7	52
Females . .	1	7	2	10	13	33
Total . . .	7	27	14	17	20	85

Average Males: 1.32 ± 0.029
Females: 1.48 ± 0.043
Diff./ ε (D) = 3.1

If one adds together cases with an index below 1.30 as leptosomes, those between 1.30 and 1.50 as athletics and those above 1.50 as pycnics, the frequencies would be about 40, 37 and 23 per cent respectively. The dividing lines of 1.10–1.30, 1.40–1.50 and above 1.50 were given by *v. Rohden* for the respective groups (cf. *Enke* [1940], p. 60).

For 57 cases who were clinically grouped, mesurements were also available. Of these 25 (44 per cent) were grouped as leptosomes (mean index 1.27 ± 0.04), 26 (46 per cent) as athletics (mean index 1.44 ± 0.03) and 6 (10 per cent) as pycnics (mean index 1.62 ± 0.11).

The agreement between the clinical evaluation and the indices which were calculated afterwards thus is rather good. According to *Luxenburger* ([1939] p. 786), one usually reckons with 50 per cent leptosomes, 17 per cent athletics and 14 per cent pycnics, the rest being "dysplastics". It would therefore seem that there is a certain shift to the right in the present material. At any rate the athletic type is probably overrepresented. If this could be of importance for the clinical picture is hard to tell, but should be considered as a possibility. In general, the people of the investigation area differ somewhat in regard to body type in comparison with people from more southern parts of Sweden. They are of smaller stature and somewhat stouter. This could be shown somewhat more objectively by a comparison of Swedish conscripts from three regions.

Through the courtesy of Professor *G. Dahlberg* I have been able to use his unpublished data on Swedish conscripts of the year 1942 for calculations of *Rohrer's* index from Norrbotten county, Älvsborg county in the lower middle part of Sweden and Malmöhus and Kristianstad counties in the extreme south. For technical reasons, the indices were calculated on the means of height and weight of each group which is not quite correct as there is a correlation between these attributes. The standard errors have been calculated according to *Dahlberg* ([1948] p. 95). The results are shown in table 13. The differences between the south and the north are statistically significant, the index being highest in Norrbotten, i.e. indicating the prevalence of a shorter and stouter type.

Table 13. Averages of *Rohrer*'s index as calculated on Swedish conscripts (20 years of age) from three different regions.

Norrbotten county	1131	1.31 ± 0.007
Älvsborg county	995	1.29 ± 0.005
Malmöhus and Kristianstad counties	1242	1.28 ± 0.005

One should not be too much impressed by figures and tables showing the distribution of different body types as the definitions are rather vague. Even if they are based on actual measurements, the interpretation of the figures is difficult and dividing lines inherently arbitrary. They could, however, be accepted as rather reasonable estimates of the different components of a group.

In summary, it seems justified to conclude that it appears that in the present schizophrenic material (and probably in the investiga-

tion area) the athletic (muscular) body type was more common than what one usually finds among unselected schizophrenics (and probably this type is more common in the investigation area than in the southern parts of Sweden).

Age of onset.

For an analysis of the age of onset one could use either the age when the first symptoms were noted according to the history of the patient or his age when first admitted to a mental hospital. Both methods involve errors. The onset is usually insidious and the patient or his informants very often cannot state exactly when his condition deserved to be considered as a disease. However, it is usually possible to refer the onset to a certain year. The latter method will, of course, give higher figures as there is always a time lag between onset and hospitalization which varies from place to place and depends on available beds, distances, financial conditions and a great many other causes all of which deprive the date of first admission of any real scientific value. The use of this method here is out of question for two reasons; firstly, quite a few cases were never hospitalized and secondly, in this area it appears that hospitalization does (or did) not occur on medical but primarily on social grounds, i.e. the patient had become so aggressive, violent or difficult to handle at home that he had to be confined in an institution, or he had committed a major crime. Of the 85 cases living and resident September 1, 1949, 67 were or had been in a mental hospital. Of these, 19 had been definitely ill more than 10 years prior to their first admission.

The age of onset in this study refers to the time when it was first noted that there was something really wrong with the patient, usually described as a change in his personality with odd behaviour, excitement, depression, apathy or some other deviation which relatively soon made him asocial and prevented him from going on with his usual activities on a full scale.

The distribution of the schizophrenics on ages of onset per 5-year groups is shown in table 14. The data have been divided in a cross section group comprising the 85 cases with a conclusive diagnosis living and resident on September 1, 1949, and a longitudinal group comprising all cases who have contracted the disease during 1902–1945. The two groups, of course, partially contain the same cases. In the latter group were included also those cases who were diagnosed as schizophrenia ?. This longitudinal group was computed because it is

better suited for the following genetic-statistic calculations. On account of what has been said above concerning the diagnosis schizophrenia ?. and the fact that no significant differences in regard to age of onset was found in comparison with the cross section group, it seemed justified to use it for that purpose.

Table 14. Schizophrenia. Age of onset.

Age group	Conclusive schizophrenics living and resident Sept. 1, 1949				All known cases of schizophrenia and schizophrenia ? with onset during 1902–1945			
	Males	Females	Total	%	Males	Females	Total	%
0 – 4								
5 – 9					2		2	1.7
10 – 14	1		1	1.2	1	1	2	1.7
15 – 19	6	5	11	13.0	9	5	14	11.7
20 – 24	17	7	24	28.2	21	11	32	26.7
25 – 29	9	9	18	21.2	11	13	24	20.0
30 – 34	9	3	12	14.1	13	3	16	13.3
35 – 39	5	2	7	8.2	3	2	5	4.2
40 – 44	3	4	7	8.2	6	5	11	9.2
45 – 49		2	2	2.4	1	3	4	3.3
50 – 54						2	2	1.7
unknown		3	3	3.5	2	6	8	6.7
Totals	50	35	85	100.0	69	51	120	100.2
Means	27.1±1.0	29.1±1.6	27.9±0.9		26.8±1.0	29.7±1.5	28.0±0.9	

In summary, it can be stated that the mean age of onset in the present material was for males about 27 years and for females about 29 years with an average for both sexes of about 28 years. In this respect, the data do not show any important differences in comparison with what is previously known about the age of onset in schizophrenia (cf. e.g. *Dahlberg* and *Stenberg* [1931], *Strömgren* [1938], *Sjögren* [1948] and *Fremming* [1947]).

Mortality.

It is well known that the mortality after the onset of schizophrenia is considerably increased. The resistance against various somatic diseases appears reduced which is probably due to somatic changes caused by the schizophrenic disease itself or psychologic responses as for instance reduction of food intake. The prominent

role played by tuberculosis has long been recognized (*Malzberg* [1934], *Kallmann* [1938], *Alström* [1942]).

The present data did not allow a sufficiently correct calculation of the mortality of the schizophrenic individuals. A crude estimate suggested that the mortality was about twice that of the Swedish rural population. Although this result appeared rather reasonable its objective value had to be considered very questionable.

For the calculation of the general morbid risk of schizophrenia in the investigation area it is, however, necessary to resort to some reasonable estimate of the excess mortality. The detailed analysis by *Alström* [1942] concerned schizophrenic individuals who were hospitalized during 1924–1937. Table 15 which has been taken from his monograph shows that the schizophrenics had experienced a mortality that was about twice that of the general population. Approximately the same result was obtained by *Malzberg* [1934] in the USA.

Table 15. Standardized death rates per 1,000 annual exposures among patients with schizophrenia and in the population of Stockholm and ratios of the corresponding death rates (according to *Alström* [1942], p. 208, table 47).

Population	Standardized death rates		
	males	females	both sexes
Schizophrenic patients 1924–36	23.8±2.2	26.8±1.8	25.7±1.3
Stockholm population 1924–36	12.7±0.07	11.7±0.06	12.1±0.05
Ratios	1.9	2.3	2.1

Whereas the above-mentioned investigations strictly deal with the mortality during hospitalization, *Essen-Möller* [1935] examined the mortality of schizophrenic hospital *propositi* inclusive of those who had been discharged. His *propositi* were taken from the Psychiatric Clinic in Munich, Germany, and their mortality was analysed for the period 1921–1930. *Essen-Möller* concluded that the mortality of the schizophrenics could be estimated at about three times that of the general population (*op.l.c.*, p. 118).

It will, of course, be granted that the results of these analyses might not be strictly valid for the schizophrenics of the investigation area. In the present material there are quite a few individuals who had never been hospitalized. It could be that such individuals display a more normal mortality. Furthermore it should be recalled that the mortality of the general population of Norrbotten county is higher as

compared to average conditions in Sweden (cf. p. 26) which might to some extent level out the differences between schizophrenics and non-schizophrenics. Considering what is known about the excess mortality in schizophrenia and with appreciation of the difficulties involved in applying such findings to the present data I have nevertheless deemed it justified to accept an excess mortality amounting to about twice the mortality of the general population as a reasonable and conservative estimate. This estimate will be used at the calculations of the influence of the excess mortality on the general morbid risk figures of schizophrenia in the present population (cf. p. 60).

Causes of death.

Although the observations are very few, it may still be of some interest to report the causes of death for those conclusive schizophrenics who are included in table 7 (p. 29). Three of the 38 cases had migrated and for the remaining 35 the distribution was as follows:

	Present data Per cent	*Alström* [1942] (774 cases) Per cent
Dis. due to old age	—	4.3
Tuberculosis	45.7	35.8
Other infections	2.9	2.1
Dis. of the nerv. syst.	—	5.4
Dis. of the circ. syst.	5.7	17.4
Pneumonia	11.4	13.2
Other respir. dis.	—	2.0
Dis. of the digest. syst.	—	5.6
Dis. of the urin. syst.	2.9	2.1
Tumors incl. of cancer	8.6	8.7
Unnatural death	—	2.3
Suicide	8.6	—
All other diseases	14.3	1.1

The similarities with *Alström*'s data are apparent. The conclusion that tuberculosis is the most important single cause of death also among the schizophrenics of the investigation area seems justified.

ITEMS OF PUBLIC HEALTH INTEREST

Hospitalization.

The duration of time from the onset of schizophrenia to the first admission to a mental hospital is shown in table 16 for all hospitalized cases with a conclusive diagnosis. From a medical view-

Table 16. Time lag between onset of symptoms in schizophrenia and first admission to a mental hospital. Cases of conclusive diagnosis.

Time lag, years	Living and resident Sept. 1, 1949		Deceased during 1901–49	
	No. of cases	Per cent	No. of cases	Per cent
0	14	20.9	3	10.0
0.5	10	14.9	2	6.7
1	9	13.4	1	3.3
2	10	14.9	8	26.7
5	5	7.5	5	16.7
10	11	16.4	4	13.3
15	1	1.5	1	3.3
20	4	6.0	1	3.3
25	1	1.5		
30	0		2	6.7
35	1	1.5	2	6.7
unknown	1	1.5	1	3.3
Totals	67		30	

point, the result of this analysis is depressing. Even for the relatively recent group of cases the number of those who got proper medical attention within one year of onset is small. There are no difficulties in offering explanations for this state of affairs. With the exception of the yearly visits of the superintendent of the Welfare Organization, there is no psychiatric service closer than about 250 km. from Pajala, the mental hospital in Piteå is over-crowded and so forth. Even if the opinions differ concerning the value of modern therapy (insulin, electroshock etc.) (*Lewis* [1950]), there remains no doubt that early treatment is of definite value for re-education and for a prevention of inveterated regressions. As mentioned previously, social, not medical causes have brought the patient to the hospital. Schizophrenia is the most important single disease in this area insofar as

incidence, destructiveness and economic loss taken together are concerned.

Of the 85 cross-section *propositi*, 18 had never had the benefit of psychiatric treatment or had been in a mental hospital.

Status of the schizophrenic on cross-section date.

The condition of the 85 cases of schizophrenia on September 1, 1949 is shown in table 17. Only 35, or 3.9 per thousand of the total population (8,981), were hospitalized. Still this "official" figure is somewhat higher than for total Sweden[1]. On December 31, 1949, there were according to official statistics 22,590 schizophrenics hospitalized or 3.2 per thousand. Within the investigation area a further 21 cases were cared for in their homes but were in need of hospitalization (2.3 per thousand). An additional 22 cases had recovered and were partly able to work but were in need of some psychiatric supervision. Only seven cases could be regarded as recovered and fully socialized.

Thus it could be estimated that the investigation area would need 6.2 beds in a mental hospital per 1,000 population for the care and treatment of schizophrenia alone. As mentioned previously, Norrbotten county has only one mental hospital in Piteå with 664 beds plus an annex in Öjebyn with 177 beds for less severe cases (figures of 1949). The total population of the county in 1949 was 239,145. If it had been given the same facilities as the rest of the country there should have been 765 beds for schizophrenics. However, the

Table 17. Status of all cases of schizophrenia September 1, 1949.

Condition		Number of cases aged			Total	Per cent per sexes
		20–24	25–65	65–ω		
Recovered and able to work	m		2		2	4.0
	f		5		5	14.3
Recovered and partly able to work	m		11	1	12	24.0
	f		9	1	10	28.6
Ill at home	m		7	2	9	18.0
	f		9	3	12	34.3
In hospital	m	1	25	1	27	54.0
	f		6	2	8	22.9
Totals	m	1	45	4	50	100.0
	f		29	6	35	100.0

[1] An exact comparison is, of course, not permissible as the age distributions are different.

point is that the need is far greater. It is, of course, impossible to tell if the investigation area is representative of Norrbotten at large. If it were, no less than about 1,400 beds would be needed for schizophrenics. Considering that the true incidence of psychotics of all types in Norrbotten could not very well be much less than the incidence of schizophrenia alone in the investigated area, 1,400 beds would in any case be more in agreement with reality than the total of 841 now available. The urgent need of an amplification of the mental health program not only in Norrbotten but in the whole country has since long been realized by all public health workers. Such a program sponsored by the government is slowly under way. I do think, however, that the latent need for care in the field of major psychiatry has been rather underestimated. Insofar as Norrbotten county is concerned, the situation is becoming increasingly untenable.

MARRIAGE RATES AND REPRODUCTION

An attempt was made to analyse two main points of interest in relation to marriage and reproduction of the schizophrenics. These items were firstly the general reproductive capacity of the schizophrenics as a group compared with the general population, and secondly the question of whether or not there were indications that the schizophrenic disease as such biologically interferes with fecundity. Both problems are of great significance from a genetic viewpoint. Also those who do not regard schizophrenia as a genetic disease will grant that the morbid risk of the children of schizophrenics is significantly higher than that expected on a random basis (cf. e.g. *Kallmann* [1938]) whereby the schizophrenics irrespective of the etiology contribute to a higher incidence of the disease among their children than other people among theirs.

A study of the reproductive capacity of the mentally diseased was carried out by *Dahlberg* [1933] who studied 2,200 females treated in a Swedish mental hospital. The principles of calculation introduced by *Dahlberg* have been of great importance for subsequent studies on this and related topics. As the data were not grouped according to different diagnoses, a comparison with this material cannot be made.

Most of the important problems in respect of marriage and reproduction of schizophrenics have been analysed in detail by *Essen-Möller* in his very thorough and sophisticated monograph of

1935. He concluded that the reproductive capacity was considerably reduced in comparison with the general population and that this was due mainly to a lower marriage rate and the early age of onset whereas the excess mortality had no appreciable significance. The data concern 2,341 schizophrenics admitted to the Psychiatric Clinic in Munich, Germany, during 1904 to 1927.

The collection of primary data for the following analysis was reported on p. 24.

Marriage rates.

Table 18 shows that schizophrenic males as well as females display a significantly lower marriage rate than a randomized sample of comparable individuals. The difference is most pronounced for the males. According to previous reports (cf. *Essen-Möller* [1935], p. 23) concerning hospitalized patients, about 25 per cent of the males and 45 per cent of the females were or had been married. *Dahlberg* [1933] found that among mentally ill females in Sweden (hospital population) 55 per cent of those above 45 years of age were or had been married. The corresponding figures for the general female population was about 80 per cent. It was not possible to group these data according to diagnoses. Directly comparable with the present data are those of *Brugger* [1931] from a census in Thuringia, Germany. He ascertained 33 male schizophrenics and 40 females. Thirty per cent of the males and 43 per cent of the females were or had been married.

Essen-Möller [1935] reported that among schizophrenics above 15 years of age, 18 per cent of the males and 30 per cent of the females were married. Exact comparisons between these different figures are

Table 18. Schizophrenia. Marriage rates. For each case a control individual who was born the same year and was living and resident in the area September 1, 1949 has been selected at random from the parish register. The schizophrenic cases are those also living and resident on September 1, 1949.

	Married	Unmarried	Per cent married	χ^2	P
Males					
Schizophrenic[1]	14	42	25.0	34.8	< 0.001
Controls	45	11	80.6		
Females					
Schizophrenic[1]	27	10	73.0	6.83	< 0.01
Controls	36	1	93.5		

[1] inclusive of probable cases.

not possible without corrections for differences in age, hospitalization and so forth. It would seem, however, that in the present material the marriage rate of the female schizophrenics is fairly high in comparison with the other data just mentioned. In any case, it is absolutely unexpectedly high. This is very likely due to the fact that the present population has been living under relatively primitive conditions where selection of a partner was influenced by such factors as physical strength and ability to share hard work rather than by minor or even considerable psychic oddities. Furthermore, there has been a constant lack of females in the area. During the last decades an increasing number of young women have migrated to other parts of the country where they hope to attain a higher standard of living and don't have to raise half a dozen children, or do farm work etc.

Reproduction.

For a calculation of the fertility only those children who were born intramatrimonially were taken into consideration. Children include live births as well as stillbirths. Twins were counted as single births. Fertility has been expressed as the average number of children born per observation year counted from the year of marriage to the age of 47 inclusive for wives and for husbands to the year when the wife reached the age of 47 inclusive. For those wives who on September 1, 1949 had not reached the age of 48 years, their (respectively their husband's) observation period ended on that date. For all schizophrenics, the time they were hospitalized in a mental institution was subtracted. After the birth of a child, 6 months were subtracted as allowance for involuntary sterility after childbirth. The results of these calculations are given in table 19.

It will be seen that the intramatrimonial fertility of the schizophrenics before as well as after the onset of the disease appears higher than the fertility of the control individuals. The differences are not

Table 19. Intramatrimonial fertility of schizophrenic *propositi.*

	Average fertility per year			
	Schizophrenic males=11	Controls males=41	Schizophrenic females=26	Controls females=34
Before onset	0.64 ± 0.065		0.53 ± 0.047	
After onset	0.48 ± 0.056		0.40 ± 0.036	
Total	0.54 ± 0.042	0.44 ± 0.024	0.43 ± 0.026	0.30 ± 0.020

statistically significant so that the only conclusion that is warranted would be that there is nothing to indicate a decreased fertility of the schizophrenics. For a comparison the main results of *Essen-Möller*'s study are repeated here. He found that before onset, schizophrenic males, in a population not practising birth control, displayed the same fertility as controls. When birth rates decrease in the population their fertility appears above average. Before onset, schizophrenic females display about 50 per cent of average fertility. As birth rates decrease the difference gradually disappears. After onset, fertility is reduced to 70 per cent of the value before onset but only in marriages which were contracted before onset.

In the present population, practically no birth control was practised, due among other things, to the prevalence of an orthodox puritanic religion (Laestadianism). The only discrepancy which appears significant in comparison with *Essen-Möller's* data would be that in this material no difference regarding fertility was found between schizophrenic females before onset and controls.

In conclusion, it could therefore be stated that no indications were found that the schizophrenic disease as such as it appears in this population interferes with fecundity.

To answer the question about the reproductive capacity, the total number of children must be taken into consideration. This question might be put thus: "How many children on the average does the schizophrenic issue for the next generation as compared with the normal individual?" To that effect the average number of children born intra- and extramatrimonially of the 85 cross-section schizophrenics was calculated and compared with their control individuals. The results are given in table 20. As a group, the schizophrenics display a propagation capacity which is reduced to about 70 per cent of the population average. This reduction appears due exclusively to the very low marriage rate of the schizophrenic males.

As a general conclusion, it appears justified to state that the analyses of marriage rates and reproduction of the schizophrenics of the investigation area indicate that the schizophrenic disease as such does not interfere with fecundity. However, the marriage rate of schizophrenic males was very low which together with the insignificant number of children born extramatrimonially had the effect that the propagation capacity of the group was somewhat reduced as compared with the average population. The effective selection against the trait schizophrenia was only moderately expressed, the reproductive fitness was estimated at about 70 per cent.

Table 20. Schizophrenia. Number of children per individual. All *propositi* with conclusive diagnosis, living and resident September 1, 1949 have been compared with randomly selected individuals. For each *propositus* one control individual of the same sex, born the same year and living and resident September 1, 1949 was selected. The comparison shows the relative differences of propagation capacity.

		Schizophrenics				Controls			
		No.	No. of children[1]	Mean		No.	No. of children[1]	Mean	
Males	a)	11	76	6.91	1.56	41	192	4.68	4.02
	b)	50	2	0.04		50	9	0.18	
Females	a)	26	155	5.96	4.91	34	156	4.59	4.66
	b)	35	17	0.49		35	7	0.20	
		85	250	2.94 ± 0.44		85	364	4.28 ± 0.39	

[1] born alive + stillborn. a) intramatrimonially. b) extramatrimonially.

CUMULATIVE RISK IN SCHIZOPHRENIA

For the purpose of the following genetic-statistic analysis, it is necessary to have an adequate estimate of the cumulative risk. The problem is to calculate what percentage of a number of individuals who sooner or later with certainty develop schizophrenia will have done so at a particular age. These figures in regard to individuals observed as *non-schizophrenic* at a particular age, and of whom we don't know if they may get the disease later on, will show how much of the risk they have passed at that age. One might also say that the figures denote in a statistic sense how much information they give as normals. Suppose a 25 year old normal male has passed 50 per cent of the risk of contracting schizophrenia. He might still get the disease. The information he supplies is not equal to one normal which it would have been if he were 60 and had passed 100 per cent of the risk but equal to one-half normal. In this way, cumulative risk figures are used to calculate the correct statistic information supplied by individuals who are observed as normals but still are within the risk zone, i.e. still experience a certain risk of contracting the disease.

The most widely known correction procedure of this kind is probably the so-called exact method of *Strömgren* [1935]. Although this method is reasonably correct, I have preferred not to use it because of an optional element connected with it. *Strömgren* used an arbitrary method to smooth the curves. However, an error might be introduced by this procedure as inequalities might have biological

causes. Instead, I have preferred to calculate the cumulative risk by adding a minor item to a method given by *Dahlberg* and *Stenberg* [1931] without any compensatory calculations (according to principles given by *Schulz* [1936, pp. 77–81]). The choice is of no practical consequence. In regard to the many sources of error involved in the observation and registration of the primary data, mathematical subtleties are unimportant. Both methods would have given practically the same result.

The principles of the mathematical procedures have been given in table 21. The average population according to males and females for the period of 1900–1945 will be found in table 6 (p. 27) and the distribution of disease onsets in table 14 (p. 44). The cumulative risks given in table 22 represent the means of the two body of data of table 14. It is granted that the number of observations is rather small but, as mentioned previously, I have preferred a fair estimate based on my own data to statistically more voluminous calculations

Table 21. Principal scheme for the calculation of passed cumulative risk in schizophrenia. Based on the principles given by *Dahlberg* and *Stenberg* [1931] and *Schulz* [1936].

Age group	Average population 1900–45 n_i	No. of disease onsets a_i	Distribution of Schizophrenics $100a_i/\sum_a$	Calculated frequency[1] $1000a_i/n_i$	Frequency-distribution $100b_i/\sum_b$	Passed cumulative risk[2] (class centrals)
0 – 4	n_1	a_1		b_1	c_1	$\frac{c_1}{2} = d_1$
5 – 9	n_2	a_2		b_2	c_2	$d_1 + \frac{c_1 + c_2}{2} = d_2$
10 – 14	n_3	a_3		b_3	c_3	$d_2 + \frac{c_2 + c_3}{2} = d_3$
.	.	.		.	.	.
.	.	.		.	.	.
65 – 70	n_{14}	a_{14}		b_{14}	c_{14}	d_{14}
$\sum$	$\sum_n$	$\sum_a$	100	$\sum_b$	100	

[1] morbid risk per 1,000 per total period of time

[2] same as age correction figure for the respective age groups. It is doubtful if this correction given by *Schulz* (1936) is quite correct. Evidently he regards c_1, c_2 etc. as total risks for the respective age groups and the correction then adjusts these risks to class centrals. Such a reasoning would have to assume that all individuals n_1, n_2 etc.of the average population had passed all the risk, which is not completely true. If one considers c_1, c_2 etc. as exact class centrals the cumulative risk should be calculated without *Schulz*'s correction. In regard to the calculation of the morbid risks in this paper the difference seems to be of no consequence.

Table 22. Schizophrenia. Passed cumulative risk for males and females of the present material. The risks refer to class centrals.

Age group	Passed cumulative risk, per cent	
	Males	Females
0 – 4	0.00	0.00
5 – 9	0.41	0.00
10 – 14	1.35	0.28
15 – 19	6.74	4.85
20 – 24	25.36	18.21
25 – 29	47.55	40.88
30 – 34	67.32	58.70
35 – 39	83.08	66.19
40 – 44	93.13	77.83
45 – 49	99.38	91.19
50 – 54	100.00	98.09
55 – 59	100.00	100.00

based on schizophrenics from other sources. The figures in table 22, however, do not differ appreciably from those given by *Strömgren* [1935] for the schizophrenics belonging to the data of *Ruedin* and *Schulz* of Munich, Germany.

INCIDENCE AND GENERAL MORBID RISK

Crude incidence.

The incidence of schizophrenia as calculated on the total population per September 1, 1949 was:

I. Schizophrenia 85/8,981 = 0.95 ± 0.10 per cent
  (i) Males 50/4,791 = 1.04 ± 0.15 per cent
  (ii) Females 35/4,190 = 0.84 ± 0.14 per cent
II. Schizophrenia + schizophrenia? 93/8,981 = 1.04 ± 0.11 per cent
  (i) Males 56/4,791 = 1.17 ± 0.16 per cent
  (ii) Females 37/4,190 = 0.88 ± 0.14 per cent

The difference between the two sexes is not statistically significant. All these figures are considerably higher than those found in any previous census. The average of those published before 1947 was 0.24 per cent (*Fremming* [1947], table 3, p. 36–37). However, the fact that in some of these studies only those individuals were ascertained who were ill on the cross-section day and the existence of differences in age distributions of the respective populations make detailed comparisons rather difficult.

General morbid risk.

More important than crude incidences are the figures expressing the morbid risk which implies the probability that a newborn subsequently will contract schizophrenia if he lives to be at least 55 or 60 years old. For a calculation of morbid risks, it is necessary to correct for age and excess mortality. In most previous studies of schizophrenia, corrections for excess mortality have been omitted. To make comparisons with such studies possible, the results of morbid risks calculations with age corrections, only, will be given separately.

The simplest method of correction is that proposed by *Weinberg* (the so-called abridged method of *Weinberg*, cf. *Schulz* [1936], p. 76). The results are given in table 23. The morbid risk calculations in regard to the population have been referred to the year 1945 (December, 31) because exact information on the age distribution of the population was not available for the years 1946–1949. It should be

Table 23. Schizophrenia. General morbid risk calculated according to *Weinberg's* abridged method. Status of the population in 1945. Cases of schizophrenia and schizophrenia? living and resident December 31, 1945.

Diagnosis	No. of cases	Risk period	Corrected population	Morbid risk per cent
A. *Males*				
Schizophrenia	44	15 – 45	1,979	2.22 ± 0.33
		20 - 45	1,603	2.74 ± 0.41
		15 - 50	1,699	2.59 ± 0.39
Schizophrenia				
+ schizophrenia? . . .	52	15 – 45	1,979	2.63 ± 0.36
		20 – 45	1,603	3.24 ± 0.44
		15 – 50	1,699	3.06 ± 0.42
B. *Females*				
Schizophrenia	39	15 – 45	1,488	2.62 ± 0.41
		20 – 45	1,309	2.98 ± 0.47
		15 - 50	1,416	2.75 ± 0.43
Schizophrenia				
+ schizophrenia? . . .	42	15 – 45	1,488	2.82 ± 0.43
		20 – 45	1,309	3.21 ± 0.49
		15 – 50	1,416	2.97 ± 0.45
C. *Both sexes*				
Schizophrenia	83	15 – 45	3,467	2.39 ± 0.26
		20 – 45	2,912	2.85 ± 0.31
		15 – 50	3,115	2.66 ± 0.29

observed that although the crude incidences for males appeared a little higher than for females, the situation was reversed insofar as the morbid risks were concerned. In no case, however, are the differences significant. Therefore it seems justified to conclude that there is no sex preference in this material.

For the calculation according to the method elaborated on the basis of the principles of *Dahlberg* and *Stenberg* [1931], the figures of accumulated risk (see table 22) have been used as correction factors. The morbid risks thus found are given in tables 24 and 25 for males and females respectively.

In table 26 the morbid risks of schizophrenia as reported in a number of different studies have been compiled. Too detailed comparisons between these figures cannot be made simply because the type of ascertainment and the diagnostic criteria have varied somewhat with different investigators. These differences and inconsistencies, however, should not be of such a magnitude that all possibilities of comparison are out of question. By and large, the figures represent

Table 24. Schizophrenia. General morbid risk for males. Calculation elaborated on the basis of principles given by *Dahlberg* and *Stenberg* [1931].

Male population of 1945 Age groups	No.	Per cent Passed risk	Corrected No.
0 – 4	640	0.00	0.00
5 – 9	648	0.41	2.68
10 – 14	555	1.35	7.48
15 – 19	388	6.74	26.14
20 – 24	399	25.36	101.18
25 – 29	337	47.55	160.25
30 – 34	298	67.32	200.61
35 – 39	296	83.08	245.91
40 – 44	235	93.13	218.84
45 – 49	196	99.38	194.78
50 – 54	147	100.00	147.00
55 – 59	132	100.00	132.00
60 – 64	109	.	109.00
65 –	236	.	236.00
Totals	4,616		1,781.87

No. of schizophrenic males 44.
Morbid risk 44: 1,782 = 2.47 ± 0.37 per cent.
No. of schizophrenic males (inclusive of probable cases) 52.
Morbid risk 52: 1,782 = 2.92 ± 0.40 per cent.

Table 25. Schizophrenia. General morbid risk for females. Calculation elaborated on the basis of principles given by *Dahlberg* and *Stenberg* [1931].

Female population of 1945 Age groups	No.	Per cent passed risk	Corrected No.
0 – 4	645	0.00	0.00
5 – 9	612	0.00	0.00
10 – 14	500	0.28	1.38
15 – 19	358	4.85	17.36
20 – 24	277	18.21	50.45
25 – 29	252	40.88	103.01
30 – 34	295	58.70	173.16
35 – 39	224	66.19	148.27
40 – 44	175	77.83	136.20
45 – 49	143	91.19	130.40
50 – 54	146	98.09	143.22
55 – 59	124	100.00	124.00
60 – 64	83	.	83.00
65 –	201	.	201.00
Totals	4,035		1,311.45

No. of schizophrenic females 39.
Morbid risk 39: 1,311 = 2.97 ± 0.47 per cent.

No. of schizophrenic females (inclusive of probable cases) 42.
Morbid risk 42 : 1,311 = 3.20 ± 0.49 per cent.

fairly adequate estimates and that is about all that can be achieved at present. The most adequate risk figure for the Danish and probably also Northwestern European and South-Swedish populations appears to be 0.90 ± 0.15 per cent (*Fremming* [1947]). Certain trends are of special interest. There remains as a possibility that the incidence of schizophrenia is lower in some South-German populations (*Brugger* [1931, 1933b and 1938], *Schade* [1950]).

The study by *Kaila* [1942] indicates that the incidence of schizophrenia in Finland is higher, if we adopt the Danish figure as a standard. The risk figure obtained in the present study is significantly higher than in all other studies. One furthermore has to consider the possibility that the incidence of schizophrenia is higher in the north than in the south of Europe. In any case, the hypothesis that schizophrenia occurs with a different frequency in different populations seems justified. There could be two major reasons for this. Either it is an effect of differences in environment or it is due to different genetic composition of the populations. These assumptions

Table 26. General morbid risk of schizophrenia as reported in studies of different types.

Data ascertained by	Corrected population (*Weinberg's abridged* method)	Schizophrenia. Morbid risk. Per cent.	Type of population
Genealogic random test method.			
Several authors. Data compiled by *Fremming* ([1947], table 1, p. 30).	6,709	0.72 ± 0.10	Average, mostly German populations
Census method.			
Brugger, 1931	18,312[1]	0.38 ± 0.05	Thuringia, Germany
Brugger, 1933b . . .	2,894[2]	0.41 ± 0.12	Allgäu, Germany
Brugger, 1938	1,643[2]	0.36 ± 0.15	Rosenheim, Germany
Schade, 1950	1,929[4]	0.52 ± 0.16	Schwalm, Germany
Strömgren, 1938 . . .	429[1]	0.47 ± 0.33	Rø, Bornholm
Strömgren, 1938 . . .	19,045[3]	0.65 ± 0.05[5]	Bornholm, Denmark
Sjögren, 1948	4,800[1]	0.83 ± 0.13	West-Swedish island
Sjögren, 1935	4,390[3]	0.68 ± 0.12	Two North-Swedish
	3,440[4,6]	0.87 ± 0.16	isolates
Kaila, 1942	194,000[1]	0.91 ± 0.02[7]	Finland
This work	3,467[4]	2.39 ± 0.26	North-Swedish
	2,912[3]	2.85 ± 0.31	isolate
Birth register test.			
Fremming, 1947 . . .	3,777[4]	0.90 ± 0.15	Bornholm, Denmark

The following risk periods were used: [1] 20–40; [2] 15–40; [3] 20–45; [4] 15–45. For comparisons these differences are not important.

[5] With correction for excess mortality. [6] Recalculated by the writer. *Sjögren* had no actual age distribution of this population but computed it according to the average Swedish rural population. As shown in this paper, the population of North Sweden differs somewhat insofar as the younger age groups are larger. This calculation was based on the assumption of the same age distribution as persisted in the investigation area in 1935 which probably gives a more correct morbid risk. [7] Does not include recovered cases. If these are taken into account, the morbid risk might be estimated at 1.15–1.20 per cent (*Kaila* [1942]).

are deliberately rather extreme over-simplifications. The problem might be tested. If, namely, the differences are mainly environmental, the same factors which increase the general morbid risk should also tend to increase the morbid risk among the relatives of schizophrenic *propositi*. If, on the other hand, the explanation is a difference in the frequency of specific disease producing genes, the latter morbid risk should be approximately the same.

As will be shown in the following, the risk figures for the parents and siblings of the schizophrenic *propositi* of this study were not appreciably higher than those found e.g. by *Kallmann* [1938, 1950].

By no means could they be about three times higher than previous figures which one would expect on the basis of the general risk being increased by that amount.

Thus one might consider these findings as another item in favour of the genetic theory of schizophrenia. The simplest explanation of the differences of the incidence of schizophrenia in different populations would be entirely in line with the behaviour of a genetic disease. As was shown above, the selection against schizophrenia in the present population has been only moderately expressed, so that its increase and at present high equlibrium is easily understandable as primarily an effect of isolation and genetic drift. It should be stressed that these explanations are merely tentative. The definition of the trait schizophrenia is still too vague to justify a detailed genetic population analysis. Furthermore, it is necessary to make the qualification that the catatonic type prevalent in the present population might not at all be comparable with the schizophrenias of other populations. Still, however, it would represent an example of a genetic disease having attained a high frequency in an isolate.

Correction for excess mortality.

As was shown above (p. 44), the mortality after onset of schizophrenia is about doubled as compared with average conditions. This fact has the consequence that the calculations of the morbid risks result in figures that are somewhat too low. The difficulties of correcting for excess mortality in connection with population studies have been pointed out e.g. by *Schulz* ([1936], p. 89). *Strömgren* [1938] tried to make a correction by including a fraction of those schizophrenics who had died prior to the cross-section day. He figured, quite correctly, that if the schizophrenics had displayed the same survival rate as the general population some of them would have survived to be included in his census. For each of them was calculated starting from the age of onset the probability (on the basis of normal survival rates) to survive and then each one was included in the census counts with that weight. With this correction, *Strömgren* obtained a general risk of 0.65 per cent instead of 0.58 per cent for the island of Bornholm (cf. table 26, p. 59). *Sjögren* [1948] also made corrections for excess mortality, raising the figure from 0.83 per cent to about one per cent. Dr. *Sjögren* informed me that he introduced here a new method of calculating the excess mortality which was published in another paper (*Sjögren* and *Larsson* [1949] p. 38–40).

I used the following method which will be submitted for further trial and criticism. The procedure is described in a general form in table 27. The observations needed are the number of onsets per year per age group of the population (not necessarily the absolute numbers but a sample that gives reasonably correct relations between the onsets of each year group i.e. a_1, a_2, a_3 etc. in the table), the normal survival or death rate per year group and finally the survival rate of the schizophrenics. The purpose is to calculate how many more schizophrenics would be living at any particular time if their survival rate after onset was normal. This is expressed in the correction quotient c_q and by multiplying the observed number of schizophrenics living at any particular time with c_q, one corrects for the excess mortality. Performing this calculation for the present material, the c_q-values were found to be 1.27 for males as well as females.

Table 27. Calculation of correction quotient (c_q) to adjust for excess mortality of schizophrenics when the general morbid risk is determined on the basis of population data. If exact data on the death rates of schizophrenics by year classes are available, these should be used in column 4 instead of an adjusted average increase. Strictly the use of this calculation should require that one is concerned with a stationary population. The result obtained here is therefore at the best a very crude estimate. Nevertheless the procedure was deemed justified as there was no other way to estimate the influence of excess mortality on the morbid risk figure.

Age group 1	Observed no. of onsets per year in the studied population 2	Calculated no. of survivors at normal death rate d_i 3	Calculated no. of survivors at α times normal death rate of each 5-year group 4
0 – 4	a_1	$a_1 (1-d_1) = b_1$	$a_1 (1-\alpha d_1) = c_1$
5 – 9	a_2	$(b_1+a_2) (1-d_2) = b_2$	$(c_1+a_2) (1-\alpha d_2) = c_2$
10 – 14	a_3	$(b_2+a_3) (1-d_3) = b_3$	$(c_2+a_3) (1-\alpha d_3) = c_3$
.	.	.	.
.	.	.	.
Totals	$\sum_{i=1}^{n} a_i$	$\sum_{i=1}^{n} b_i$	$\sum_{i=1}^{n} c_i$

$$\text{Correction } c_q = \frac{\sum_{i=1}^{n} b_i}{\sum_{i=1}^{n} c_i}$$

As no age-specific death rates were available for the investigation area, the figures for Norrbotten county were used. Furthermore, it was counted with a twice normal death rate throughout after onset of schizophrenia ($a = 2$). This correction would raise the lowest calculated morbid risk to 2.82 per cent and the highest to 3.78 per cent (conclusive cases, only, included). When using the abridged method of *Weinberg*, it seems most correct to use a risk period of 15–45 years. If only conclusive cases of schizophrenia were taken into account, this procedure gave a morbid risk of 2.39 per cent. The method of *Dahlberg-Stenberg-Schulz* gave 2.47 per cent for males and 2.97 for females. The average of these estimates is 2.61 per cent. With the above correction for excess mortality, this figure will be raised to 3.31 per cent. On the basis of all these calculations, it was decided to consider a risk figure of 3 per cent as a reasonable and conservative estimate.

In summary, it might be concluded that concerning the type of schizophrenia of the investigation area the crude incidence was approximately one per cent and the morbid risk approximately three per cent, with no appreciable differences between the two sexes.

MORBID RISK OF PARENTS AND SIBLINGS OF SCHIZOPHRENIC PROPOSITI

For reasons mentioned previously (p. 21), the definition of a *propositus* for the genetic analysis was a schizophrenic individual with a conclusive diagnosis who on September 1, 1949 was living and resident of the investigation area. These *propositi* totalled 85 cases. Five cases had to be omitted because they had immigrated from nearby areas and their families could not be investigated.

The remaining 80 *propositi* belonged to 71 parent-sibship combinations totalling 727 individuals (inclusive of the *propositi*). Among these are a further 23 cases of whom 10 were diagnosed as conclusive schizophrenias and 13 as schizophrenia ?. A summary of the most important information about these 13 cases is given in table 28. I feel satisfied that all these psychoses were true schizophrenias although some of them did not fulfill completely the criteria mentioned on p. 33. Concerning two cases (80/49 F. A. I. and 244/49 I. L. U.), the description indicated the presence of all five cardinal signs but as the information was obtained through relatives, only, and the psychoses occurred more than 20 years ago, I hesitated to classify them as conclusive. The obvious schizophrenic components of all

Table 28. A summary of the most important diagnostic data of all cases evaluated as schizophrenia? and accounted for in the genetic analysis.

Case no.	sex	Main symptoms 1	2	3	4	5	Contributory symptoms C1	C2	C3	C4	i	ii	iii	iv	Source of information
Parents:															
35/49 E.J.P.	○	+				+	+			+	+		45	ch	relatives
80/49 F.A.I.	□	+	+	+	+	+				?			46	ch	relatives
04/49 S.H.K.	○	+	+	+		+				+			84	ch	relatives
50/49 I.K.M.	□	+	+	+		+				+	+		80	ch	relatives
93/49 J.H.L.	□	+			+	+					+		81	e	relatives
86/49 M.U.	○	+		+		+					+		72	ch	distr. phys.'s report
91/49 J.F.K.	□		+	+	+	+				+			1	e	relatives + pers. exam.
Siblings:															
50/49 G.A.L.	□	+	+		+	+							26[1]	ch	relatives
58/49 O.A.V.	□			+	+	+	?				+	+	1	ch	hospital files
04/49 I.E.T.	○	+	+	+		+							50	ch	relatives
20/49 A.L.S.	○	+	+	+		+	?	+		?	+	+	1	e	hosp.file + pers. exam.
44/49 M.G.U.	○	+	+	+		+							29	ch[2]	relatives
44/49 I.L.U.	□	+	+	+	+	+	+						32	ch[2]	relatives

Explanations. Main symptoms 1 through 5 and contributory symptoms C1 through C4 refer to the description on p. 33. Positive statement of a symptom is denoted by +.

(i) Reported as insane in parish register.
(ii) Patient has been hospitalized.
(iii) Age at death, 1 = living.
(iv) Course of disease (ch) chronic, (e) episodic.
[1] Suicide.
[2] Extreme deterioration.

these cases, the absence of psychogenic causes that the informants were aware of, the lack of manic-depressive signs together with the fact that manic-depressive psychosis was practically absent within the area was considered enough reason to include both types of cases on an equal basis in the genetic analysis. A subdivision of the morbid risks as to include either conclusive cases alone or both types together does not seem justified as the majority of cases diagnosed schizophrenia? have been referred to this group on account of lack of some positive criteria and not because of positive information indicating some other type of psychosis. In the following, the concept of schizophrenia will be used to include both types of cases if not otherwise stated.

In the genetic analysis, the 23 cases just mentioned have been counted as secondary cases (cf. the definition of *propositus*). These individuals had either died or migrated after having contracted the disease before September 1, 1949. Two cases were living and resident

(291/49 J. F. K. and 120/49 A. L. S. in table 28) but not counted as *propositi* since their diagnoses were not definite.

In the analysis, schizophrenics were scored against non-schizophrenics. These latter were not necessarily "normal" from a neuropsychiatric point of view. It seems very likely that the major genic factor which no doubt is primarily responsible for the development of a schizophrenic psychosis (cf. *Kallmann* [1938], *Essen-Möller* [1941]) in regard to its action may be greatly modified or perhaps completely suppressed by extra-genic factors insofar as psychic symptoms are concerned. There appears to be a gliding scale from full-blown psychosis over abnormal personality traits (mostly so-called schizoid psychopaths or personalities) to normality. In other words, the penetrance of this anticipated major genic factor could not be 100 per cent. This penetrance enters the genetic analysis. Thereby it is not necessary to account for all diverse manifestations of the major genic factor. However, the penetrance must be referred to a

Table 29. Schizophrenia. Survey of the total number of individuals evaluated in the genetic analysis. There are no double counts in this table. Figures in brackets show the number of individuals who were personally examined. The rest were investigated as described on p. 23. The parent who is also a *propositus* had immigrated and no sibship data were available.

	Total no. of individuals	Schizophrenics		Non-schizophrenics		
		Propositi as defined. Living	Secondary cases as defined	Living	Dead	Migrated
Parents . .	142	1	13	31	95	2
1 sibship. Siblings from schiz. × schiz. matings . .	8	2	1	0	4	1
12 sibships. Siblings from schiz. × non-schiz. matings	99	14	3	38	23	21
58 sibships. Siblings from non-schiz. × non-schiz. matings . .	478	64	6	183	136	89
Totals	727	81 (72)	23 (2)	252 (123)	258 (8)	113 (8)

specific condition which is here a schizophrenic psychosis as described in this paper. Other conditions have not been entered in this analysis.

Table 29 gives a survey of the total number of individuals who were included in the genetic analysis. As we are concerned with an unconditioned representative series, the analysis was performed according to *Weinberg's* method implying that parents and sibships have been taken into account as many times as they were correlated with a *propositus*.

Parents.

The calculation with age correction according to *Weinberg's* abridged method is shown in table 30. The morbid risk of parents of schizophrenic *propositi* in this material was 12.0 ± 2.7 per cent.

Table 30. Incidence of schizophrenia among parents of schizophrenic *propositi*. Complete information of 71 families in which at least one schizophrenic child (*propositus*) was living and resident in the area September 1, 1949.

Observed parents	No.	Deceased normals	Schizophrenia living	Schizophrenia dead
Fathers	80	57	2	5
Mothers	80	50	3	8
Totals	160	107	5	13
			18	

Calculated according to *Weinberg*'s abridged method (risk period 15–50 years) the frequency will be $\frac{18}{149.5} = 0.1204$ or 12.04 ± 2.66 per cent.

Siblings.

The sibships were divided into three groups as to whether one, both or neither of the parents were schizophrenic. Except for the single sibship with two schizophrenic parents, the data were analysed with the corrective measures of *Weinberg* (abridged method), *Dahlberg-Stenberg-Schulz* and morbidity statistics (cf. *Schulz* [1936], p. 85). As the counts were made according to *Weinberg's* method, the numbers in the following tables (31 through 35) are not individuals but experiences. The differences between the number of individuals and experiences are, however, so small that a special correction of the standard errors was found to be of no significance.

Table 31. Schizophrenia. Morbid risks for siblings in the two parental combinations, unaffected × unaffected and affected × unaffected. Calculation according to *Weinberg*'s abridged method and *Weinberg's* sib method. Primary data: all cases of schizophrenia living and resident in the area on September 1, 1949, with complete information about family members.

Parental combination	Total no. of unaffected sibs	Risk period	Corrected number of sibs	Number of schizoph. sibs	Morbid risk Per cent
Both parents non-schizophrenic (64 propositi) . . .	452	15 – 45	234.5	20	8.52 ± 1.82
		20 – 45	210.5	20	9.50 ± 2.02
		15 – 50	222.0	20	9.01 ± 1.92
One parent schizophrenic, the other non-schizophrenic (14 propositi) . . .	102	15 – 45	60.0	7	11.66 ± 4.14
		20 – 45	54.0	7	12.96 ± 4.57
		15 – 50	55.0	7	12.72 ± 4.49

Both parents non-schizophrenic. The results of the analyses are given in tables 31, 32 and 33. The different analyses showed good agreements and the morbid risk of about 9 per cent appears to be an adequate estimate.

One parent schizophrenic. The results are given in tables 31, 34 and 35. Agreements were fairly good and a morbid risk of about 12 per cent would appear to be an adequate estimate.

Both parents schizophrenic. Only one sibship of 7 individuals was observed. This contained 2 *propositi* and one secondary case.

The expectancy of schizophrenia was also calculated according to *Dahlberg*'s so-called later sibling method. For sibships with two non-schizophrenic parents, the morbid risk was 7.7 ± 2.6 per cent and if one parent was schizophrenic 17.9 ± 7.3 per cent. The morbid risks for siblings born after the *propositus* thus do not differ significantly from the risks for the total number of siblings.

Among the schizophrenic parents, 8 were females and 6 males. A special analysis of the morbid risks of female siblings scored against male siblings did not reveal any differences.

Furthermore, an analysis was performed as to examine whether there would be significant differences between the three sibling groups and the population regarding morbid risks. A chi-square analysis

Table 32. Schizophrenia. Morbid risk for siblings of schizophrenic *propositi* if both parents are non-schizophrenic. The same primary data as in table 29. The sibships have been counted in accordance with *Weinberg's* sib method. Calculations elaborated on principles given by *Dahlberg* and *Stenberg* [1931].

Age intervals at the end of observation	Number of siblings	Per cent passed risk	Corrected number of unaffected siblings
0 – 4	88	0.00	0.00
5 – 9	10	0.21	0.02
10 – 14	10	0.81	0.08
15 – 19	48	5.79	2.78
20 – 24	59	21.79	12.85
25 – 29	47	44.21	20 78
30 – 34	32	63 01	20.16
35 – 39	31	74 64	23.14
40 – 44	42	85 48	35.90
45 – 49	25	95 28	23.82
50 – 54	17	99.05	16.84
55 – 59	15	100.00	15.00
60 – 64	18	100.00	18.00
65 – 69	6	100.00	6.00
70 – 74	3	100.00	3.00
75 –	1	100.00	1.00
Totals	452		199.38

Number of observed schizophrenics among the sibs 20

$$\text{Morbid risk} = \frac{20}{199.38 + 20} = 0.0911$$

$$= 9.11 \pm 1.94 \text{ per cent.}$$

gave the following significant results (the observed figures can be extracted from data given in the tables of this paper):

Siblings *versus* population: Chi-square 45.8, $P < 0.001$

Parents *versus* population: Chi-square 30.9, $P < 0.001$

Siblings with two non-schizophrenic parent *versus* population: Chi-square 22.6, $P < 0.001$

Siblings with one schizophrenic parents *versus* population: Chi-square 15.4, $P < 0.001$

Siblings with one schizophrenic parent *versus* siblings of two schizophrenic parents: P (exact) = 0.004.

Table 33. Schizophrenia. Morbid risk for siblings of schizophrenic *propositi* if both parents are non-schizophrenic. Primary data: all *propositi* with conclusive diagnosis and complete information about parents and siblings, living and resident in the area September 1, 1949, a total of 64 of whom 2 had no siblings. Calculation according to morbidity table. The sibships have been counted in accordance with *Weinberg's* sib method.

Age intervals at the end of observation	Number of siblings	Disappeared from observation Deceased or moved	Contracting schizoph.	Total	Observed for schizoph.	Thereof non-schizoph.	Relation between non-schizoph. and observed
	a	b	c	d	e	f	f/e
0 – 4	472	88	0	88	428.0	428.0	1.0000
5 – 9	384	9	0	9	379.5	379.5	1.0000
10 – 14	374	6	0	6	371.0	371.0	1.0000
15 – 19	364	42	1	43	343.0	342.0	0.9970
20 – 24	315	49	6	55	290.5	284.5	0.9793
25 – 29	250	25	6	31	237.5	231.5	0.9747
30 – 34	197	11	4	15	191.5	187.5	0.9791
35 – 39	161	6	1	7	158.0	157.0	0.9937
40 – 44	129	4	2	6	127.0	125.0	0.9843
45 – 49	85	2	0	2	84.0	84.0	1.0000
50 – 54	60	1	0	1	59.5	59.5	1.0000
55 – 59	43	1	0	1	42.5	42.5	1.0000
60 – 64	28	2	0	2	27.0	27.0	1.0000
65 – 69	10	1	0	1	9.5	9.5	1.0000
70 – 74	4	0	0	0	4.0	4.0	1.0000
75 –	1	1	0	1	0.5	0.5	1.0000

Result: Probability of *not* contracting schizophrenia 0.9114
Probability of contracting schizophrenia, morbid risk 0.0886.

However, no difference could be established between siblings with two non-schizophrenic parents *versus* siblings of one schizophrenic parent: Chi-square 0.52, $0.30 < P < 0.50$.

In all scores there was one degree of freedom.

In summary, it might be stated that the morbid risks as calculated for parents and siblings were significantly higher than the general morbid risk in the area. Furthermore, the risk appears higher for siblings with two schizophrenic parents versus siblings with one or none schizophrenic parent, whereas the risks for the two last-mentioned groups did not differ significantly.

For the subsequent genetic analysis, the following morbid risks will be adopted as fairly reasonable estimates:

Parents of schizophrenic *propositi:* 12 per cent.

Siblings of schizophrenic *propositi* of two non-schizophrenic parents: 9 per cent.

Siblings of schizophrenic *propositi* of one schizophrenic and one non-schizophrenic parent: 12 per cent.

Table 34. Schizophrenia. Morbid risk for siblings of schizophrenic *propositi* if one parent is schizophrenic and the other non-schizophrenic. The same primary data as in table 29. The sibships have been counted in accordance with *Weinberg's* sib method. Calculations elaborated on principles given by *Dahlberg* and *Stenberg* [1931].

Age intervals at the end of observation	Number of siblings	Per cent passed risk	Corrected number of unaffected siblings
0 – 4	18	0.00	0.00
5 – 9	2	0.21	0.00
10 – 14	1	0.81	0.01
15 – 19	12	5.79	0.70
20 – 24	11	21.79	2.40
25 – 29	11	44.21	4.86
30 – 34	8	63.01	5.04
35 – 39	6	74.64	4.48
40 – 44	8	85.48	6.84
45 – 49	10	95.28	9.53
50 – 54	7	99.05	6.93
55 – 59	1	100.00	1.00
60 – 64	5	100.00	5.00
65 – 69	2	100.00	2.00
70 – 74			
75 –			
Totals	102		48.79

Number of observed schizophrenics among the siblings 7

$$\text{Morbid risk} = \frac{7}{48.79 + 7} = 0.1254$$

$$= 12.54 \pm 4.43$$

CONSANGUINITY

The consanguinity between the parents of all schizophrenic *propositi* with a conclusive diagnosis (cross-sectional as well as longitudinal *propositi*) was as follows:

	Degree of consanguinity						Total
	2:2	2:3	3:3	3:4	4:4	4:5	0:0	
Number of families	8	0	7	2	7	3	91	118

All relationships have been established through the objective genealogies obtained from parish registers. The parental couples have been counted once for each *propositus* separately. The incidence of first cousin marriages was 6.8 ± 2.3 per cent, which does not differ significantly from the figure of 2.2 ± 0.4 per cent as reported previously as an average of the investigation area (*Böök* [1948]). Furthermore, it should be noted that this latter figure refers to the total number of existing marriages with both partners alive on June 15, 1947. The parents of the schizophrenic *propositi*, on the average, belong to marriages contracted about a generation earlier and thus to a period

Table 35. Schizophrenia. Morbid risk for siblings of schizophrenic *propositi* if one parent is schizophrenic and the other non-schizophrenic. Primary data: all schizophrenic *propositi* with conclusive diagnosis and complete information about parents and siblings, living and resident in the area September 1, 1949, a total of 14. Calculation according to morbidity table. The sibships have been counted in accordance with *Weinberg*'s sib method.

Age intervals	Number of siblings	Disappeared from observation Deceased or moved	Contracting schizoph.	Total	Observed for schizoph.	Thereof non-schizoph.	Relation between non-schizoph. and observed
	a	b	c	d	e	f	f/e
0 – 4	109	18	0	18	100.0	100.0	1.0000
5 – 9	91	2	1	3	90.0	89.0	0.9888
10 – 14	88	1	0	1	87.5	87.5	1.0000
15 – 19	87	8	2	10	83.0	81.0	0.9759
20 – 24	73	11	2	13	67.5	65.5	0.9703
25 – 29	60	6	1	7	57.0	56.0	0.9824
30 – 34	48	2	1	3	47.0	46.0	0.9787
35 – 39	39	1	0	1	38.5	38.5	1.0000
40 – 44	33	1	0	1	32.5	32.5	1.0000
45 – 49	25	2	0	2	24.0	24.0	1.0000
50 – 54	15	0	0	0	15.0	15.0	1.0000
55 – 59	8	0	0	0	8.0	8.0	1.0000
60 – 64	7	2	0	2	6.0	6.0	1.0000
65 – 69	2	0	0	0	2.0	2.0	1.0000
70 – 74	0	0	0	0	0.0	0.0	1.0000
75 –	0	0	0	0	0.0	0.0	1.0000

Result: Probability of *not* contracting schizophrenia 0.9002
Probability of contracting schizophrenia, morbid risk 0.0998

when the isolate nature of the area was very likely much more pronounced. One therefore has to assume that the frequency of cousin marriages in the population was higher at that time. This assumption is supported by the fact that the geographically most isolated part of the area, the parish of Muonionalusta, as late as in 1947 displayed an average incidence of 6.8 ± 1.8 per cent (*Böök* [1948]).

DISTRIBUTION ON BIRTH RANKS

The analysis of the distribution of all cross-sectional and longitudinal conclusive schizophrenic *propositi* was performed according to the principles of *Weinberg-Schulz-Böök* (cf. *Böök* and *Rayner* [1950], p. 79–81). Single born *propositi* were excluded from the calculations as they give no information. Firstborns scored against lastborns gave the corrected observed figures of 47.5 against 32.5. The uncorrected values were 13.8 ± 3.3 per cent firstborns and 8.3 ± 2.6 per cent lastborns. Thus primogeniture or ultimogeniture apparently cannot be claimed as having a connection with the schizophrenic psychoses of this material.

Table 36 gives the Chi-square test of the birth rank distribution. The deviations from the calculated expected figures are on the whole not statistically significant. *There is thus no objection to the assumption of a random distribution on the different birth ranks.*

Table 36. Distribution of schizophrenic *propositi* on different birth ranks.

Birth rank	Observed no.	Expected no.	$(o - e)^2/e$
1	15	15.6	0.02
2	15	15.6	0.02
3	15	14.1	0.06
4	22	12.5	7.22
5 – 6	19	21.7	0.34
7 – 16	23	29.3	1.35
Totals	109	108.8	9.01

Chi-square = 9.01; DF = 5; $0.10 < P < 0.20$

THE GENETIC THEORY OF SCHIZOPHRENIA

Pathogenetic and etiologic research.

The enormous difficulties involved in any research work on schizophrenia should be appreciated. The accumulated data on this

condition are so extensive that no single individual could claim to master them in details. They range from psychodynamics all the way to intra-cellular biochemistry. The picture is one of stimulating controversies and confusions. So much do we know for sure, however, that in regard to the cause of schizophrenia, no common denominator has been found or at any rate universally accepted. One of the main functions of the research worker who wants to enter this field is to be willing to take the risk of being wrong, in making concrete hypotheses which can be tested and criticized.

The diagnosis of schizophrenia is based on a constellation of purely psychopathologic symptoms. What chain of events causes the development of this psychopathologic phenotype is not known. We do not even know if schizophrenia is primarily a disease of the brain or of some other structure of the body. The discussion will be restricted to such cases of schizophrenia which today are generally accepted as such, i.e. the hebephrenic, catatonic, paranoid and simplex types. The argument whether or not these should be considered organic or functional is meaningless. In complete agreement with *Cobb* [1952], I believe the following three points are important and I quote from his summarizing remarks at the conference on "Mental Health and Disease" held at the New York Academy of Medicine in November, 1950: "I would like to make three assumptions, which I believe can be upheld by scientific data in 1951:

> First: No biological process goes on without change of structure.
> Second: Whenever the brain functions there is organic change.
> Third: The brain is the organ of mind."

If we accept these assumptions the question is no longer *if* the schizophrenic disease is connected with structural changes but of what kind these changes are. Although, as just mentioned, no common denominator has been found, structural and biochemical changes have been described in schizophrenics. Thus, recently *Winkelman* and *Book* [1949] studied the brains of 10 schizophrenics and found the following microscopic pathology: "focal and general loss of nerve cells, especially in the anterior half of the brain; the presence of numerous nerve cells showing degenerative changes, such as shrinkage, vacuolization of cytoplasm, "ghost cells", loss of polarity, and fatty infiltration. A fairly uniform hyperplasia and hypertrophy of macroglia was noted. A diffuse mild subcortical demyelinization was present". The material was taken at autopsy.

Three of the cases had been subjected to electroshock and insulin treatments. The remaining 7 cases who had had no treatment showed the same structural changes. Only two individuals were above 50 years at death. *Papez* and *Bateman* [1949] reported a study of brain biopsies taken at prefrontal lobotomies of 42 schizophrenic subjects. A special stain for desoxyribonucleic acids in nuclei was used. The pathology was interpreted as increased production of ribonucleic acids with subsequent cytoplasmic breakdown and formation of "inclusion bodies". Stages of nuclear repair and disintegration were intermingled. The pathology thus described is not in any way specific for schizophrenia. Similar changes have been found in a number of other conditions. These studies remain open to criticism. A sufficient and convincing material of normal brains for comparison was not presented. It is possible that the microscopic abnormalities so far described in the brains of schizophrenics lie within the range of normal variation. *Wolf* and *Cowen* [1952] in summing up their view of our present knowledge of this topic state: "One cannot deny that there may be an organic basis for schizophrenia and other psychoses of unknown origin but if there be, there is at present no reliable histologic evidence for it."

As pointed out by *Winkelman* [1952], the microscopic picture ascribed to schizophrenia (especially hebephrenic and catatonic types) is not a startling one. Neither does one have to anticipate a specific pathology which would coincide with the view that schizophrenia would not be primarily a disease of the brain (cf. *Luxenburger* [1939]) but rather a disease with secondary metabolic effects on that organ. The general histologic picture of disintegration and dying off of cerebral nerve cells that has been described in schizophrenic brains could be "normal" insofar as similar processes go on in every brain. However, the normal brain can take these losses as long as they don't endanger the reserve capacity. Heavy losses from known intoxications (e.g. alcohol) or infections exterminating a vast number of neurons invariably produce psychopathologic symptoms. Also at high ages the accumulated "normal" losses invariably produce psychic symptoms, naturally with considerable individual variations. The difficulty in interpreting the histologic findings in schizophrenia might be due to the possibility that we are not concerned with any specific, i.e. qualitatively different, pathology but with a quantitatively different (i.e. increased rate of) annihilation of cerebral neurons. Such differences would be very difficult to reveal, first, because of technical difficulties of quantitative measurements

and secondly, because the brain neuron reserve capacity probably is subject to individual variability.

If the pathology is non-specific and if a certain amount of damage causes symptoms, how does this view coincide with the concept of schizophrenia as a clinical syndrome? Such questions cannot be answered at present but some comments could be made. The psychic symptoms which we call schizophrenic are actually as unspecific as the supposed neuropathology. The way the brain reacts in psychopathologic terms on diffuse neuronal damage is probably rather monotonous. It goes on working rather effectively on less differentiated levels whereas the damage prevents the individual more or less to engage himself in work or normal social intercourse. What goes on in the brain produces the psychic symptoms which, of course, may vary considerably depending on the extent and localization of non-specific lesions. These views are supported by the fact that perfectly clear schizophrenic symptoms occur in conditions with visible organic pathology. Every psychiatrist is familiar with cases of general paresis who at some stage of the disease could not be differentiated from schizophrenics by any other examination than the Wassermann test. There are cases of convolutional atrophy (*Pick's* disease) who, until their post mortem, were considered conclusive schizophrenics, there are individuals with brain tumours displaying schizophrenic symptoms etc.

It should perhaps be pointed out that it has not been anticipated that the possible histologic changes in schizophrenia should be irreversible at all stages. The fact that some schizophrenics recover thus is no argument against the views expressed here.

So far, we have a possible non-specific neuropathology at the one hand and at the other a psychopathologic syndrome which can be kept together to a certain extent only by differential exclusion and clinical evaluation through extended observation. If there is a casual connection between these two, it would fit the concept of schizophrenia as primarily a metabolic disease. Biochemical studies, however, appear as controversial and difficult to interpret as do the histologic. Practically every item studied so far has shown a greater variability in schizophrenics as compared with normals (cf. *Hoskins* [1946]). No specific findings have been reported and when pathologic findings were found in some individuals, it has been impossible to decide wether they should be interpreted as causes or effects of the disease. The same judgement must be passed on the studies of

Gjessing [1932 and 1935] which are of special interest in connection with the present study as they deal with catatonics. *Gjessing* found during an intense study of 14 catatonic males a nitrogen retention between periods of stupor or catatonic excitement and an increased nitrogen output during such periods. The number of individuals studied is too small to warrant any general conclusions and as far as I am aware, *Gjessing*'s experiments have not been repeated. They do indicate, however, that metabolic alterations could be correlated with periodic changes in the clinical picture.

The psychodynamic concept of schizophrenia as a fixation at an early oral erotic level, as a conflict between the ego and the outer world, as a withdrawal of the libido from the environment and so forth is by and large a translation of the schizophrenic symptomatology into a different language. More important is the claim that the entire process is environmental and brought about by adverse psychic influences, broadly speaking in the form of various kinds of stress. There are, however, rather facts against this view as no evidence was found that the incidence of psychotic illness was raised during the different kinds of stresses that occurred in various countries during World War II inclusive of Nazi-occupied Denmark (*Svendsen* [1952]). An explanation of psychosexual traumata in infancy is, of course, still possible but such opinions are hardly open to objective arguments. It is necessary to point out that the objection is exclusively against the psychodynamic hypothesis that psychic traumata are the *only* cause of schizophrenia implying that any individual subjected to such experiences would become a schizophrenic and *not* against the importance of environmental factors as possible precipitating and contributive causes.

In summary, it might be stated that the pathogenesis of the schizophrenic syndrome is entirely unknown. Neuropathologic, biochemical as well as other somatic approaches and psychodynamic researches all hold important suggestions for further inquiries. Consequently, the question whether schizophrenia is a pathogenetic entity cannot be answered.

Genetic studies on schizophrenia.

The schizophrenic syndrome has been subjected to a large number of genetic studies. There will be made no attempt here to review even in a condensed form all these contributions, many of which have been performed with insufficient genetic and statistic methods. Instead, I shall try to outline the problem, emphasize

important observations for as well as against the hypothesis of a genetic etiology, summarize the concept of today and finally try to fit the observations of the present study into the general picture.

Let it be said from the onset that no serious geneticist has ever claimed that genetics offers the patent solution of schizophrenia and that the *clinical* genetics of schizophrenia has nothing whatsoever to do with eugenics. The often violent attacks on psychiatric genetics by psychoanalysts (cf. *Rosenberg* [1944]) hardly appear justified as by definition genetics deals always with nature *and* nurture. When the genetic theory of schizophrenia is spoken of as "the grim alternative between fatalistic nihilism and recourse to the drastic, controversial, and doubtfully efficacious measure of sterilization" (*Stern* [1941]), this reflects a misconception of genetic research work. A much more moderate view was expressed by *Gerard* and *Siegel* [1950] who studied the family backgrounds of 71 male schizophrenics. Their study indicated that the schizophrenics had been exposed to a specific background which "with respect to the constitutional and genetic theories of schizophrenia *may be* the x-factor which is necessary to develop the phenotype of the disease. On the other hand, it is equally cogent to assume that this family background is sufficient to provoke the marked distortions of personality, the weakened ego, and the potentiality for extreme anxiety we name schizophrenia". Psychiatric geneticists generally (with the notable exception of *Kallmann* e.g. [1950]) do not mention the psychodynamic hypotheses in their writings. However, in view of the importance of the psychoanalytic school of thought, especially among psychiatrists in the USA, and its great impact on research in the field of neuroses and general psychiatry, I feel obliged to state that the geneticist cannot afford to ignore psychodynamics nor can the psychoanalyst afford to ignore genetics.

The etiology of schizophrenia is a multidimensional problem and the genetic approach is just one way of trying to find out something about its nature. The first question asked by the geneticist would be: does *any* individual possess the potentialities to develop this disease or only individuals with specific discontinuously distributed genetic prerequisites? In other words, is schizophrenia a genetic disease? The extensive twin studies by *Kallmann* [1950] comprising 953 pairs of which 268 were monozygotic leave no reasonable doubt that this question should be answered in the affirmative. His concordance figures were for monozygotic pairs 86.6 ± 2.7 per cent and for dizygotic

14.5±1.7 per cent. Even if his diagnostic classification might be questioned insofar as the concept of schizophrenia was extended to include the simple, atypical and "schizo-affective" varieties showing only a very slow tendency to deterioration, the acute confusional states precipitated by extreme stress, and the "pan-neurotic" borderline cases without delusions and hallucinations described by *Hoch* and *Polatin* [1949] as the pseudoneurotic type of schizophrenia, this would not affect the very appreciable difference between the two categories of twin siblings. Another question is whether or not the about 85 per cent concordance of monozygotic twins should be considered to reflect the penetrance of an anticipated genic factor. Conclusions must be guarded on this point. First one must ask: penetrance in relation to what clinical condition? Until more detailed data have been published by Dr. *Kallmann*, there remains a feeling that many of his atypical or borderline cases would not have been judged as schizophrenic psychoses by European psychiatrists but probably as schizoid personalities with various types of psychic insufficiencies. Thus it is possible that the penetrance in regard to the schizophrenic psychosis would appear lower if more restricted diagnostic criteria were applied. This assumption is supported by a very small but intensely studied schizophrenic twin series reported by *Essen-Möller* [1941]. Of seven monozygotic pairs none was concordant in regard to a pronounced schizophrenic psychosis occurring in the twin sibling. Five twin siblings, however, displayed psychotic episodes with some schizophrenic symptoms. Furthermore, all pairs were concordant regarding schizoid personality traits which *Essen-Möller* interpreted as a more direct expression of the genic factor for schizophrenia. It is thus apparent that the concordance figure has no precise meaning unless referred to a specific condition.

Secondly: other factors tend to increase the similarity between monozygotic twin siblings. Due to their genetic identity, they share not only the anticipated specific genic factor for schizophrenia but also a number of other genes which may exert a modifying influence on the penetrance. Finally, their genetic identity makes them choose a more similar environment than dizygotic twin siblings and in as much as this environment contains precipitating factors for one of them (the schizophrenic *propositus*) it also does so for the twin sibling.

These necessary qualifications, however, do not invalidate the conclusion that some specific genic factor apparently is a prerequisite for the development of a schizophrenic psychosis. It does not seem

reasonable to assume that similarities or dissimilarities in the environment would account for the total variation observed between and within monozygotic and dizygotic twins. Although theoretically possible, no such evidence has been found as yet concerning schizophrenia. Until such data have been provided by opponents, there will be no reason to throw either the twin method or the genetic theory of schizophrenia overboard.

Another important implication of the twin studies is that in spite of the presence of specific genetic potentialities, an individual would *not* necessarily develop a schizophrenic psychosis.

On the basis of the theory that the schizophrenic syndrome is one of the phenotypical expressions of a specific genic factor, further inquiries have to be made concerning the distribution of this syndrome in populations and among the relatives of schizophrenic *propositi*. A number of important problems arise, such as: does schizophrenia occur with a significantly higher incidence among the close relatives of schizophrenic *propositi*? If so, can the distribution among these relatives be interpreted so as to indicate the prerequisite of a major gene difference? If so, does one single major gene cause all the different clinical phenotypes or do we have to assume the existence of several major gene differences which are related to different clinical phenotypes or, finally, do we have to assume several major gene differences which under different circumstances cause more or less identical phenotypes? All these questions are much more important than what is generally supposed to be the geneticist's first question, namely: how is schizophrenia inherited? Medical genetics can be productive insofar as etiologic and pathogenetic research is concerned only if it aims at an identification of specific disease producing genic factors which then could be subjected to further analyses by biochemical, histologic and other methods in order to unravel basic phenogenetic processes. These latter approaches should be more profitable if applied to biologically more homogeneous material.

Through the studies of a large number of investigators, among whom should be mentioned especially *Schulz* [1932] and *Kallmann* [1938, 1946 and 1950], it has been sufficiently established that the morbid risk of schizophrenia is much higher among the close relatives of schizophrenic *propositi* as compared with the average morbid risk in the general population.

The risk figures found for parents, siblings and children of schizo-

phrenics, which naturally vary somewhat with different types of ascertainment and diagnostic requirements, appear high enough to indicate the participation of a major gene difference (cf. *op. l.c.* and *Sjögren* [1935 and 1948], *Strömgren* [1938], *Weinberg* and *Lobstein* [1943]). This important information should not be obscured by the fact that different investigators disagree concerning the more detailed genetic interpretation of the data.

The question of the genetic homogeneity of the schizophrenic subgroups according to *Kraepelin*'s concept was subjected to an analysis by *Schulz* [1932] who concluded that his data did not furnish sufficient evidence to decide one way or the other. A comparison of the morbid risks for siblings showed that this risk was highest for sibs of hebephrenic *propositi* (19.5±4.9 per cent) whereas for the other sibling groups the figures varied between 5.6±1.9 and 9.4±1.4 per cent with an average for all groups of 8.2 per cent. The deviation shown by the hebephrenic group was not statistically significant. Although the groups were very small, *Schulz* found a significant correlation between the clinical features of the schizophrenic siblings and the subgroup to which their respective *propositi* belonged. Among the affected sibs of hebephrenic *propositi*, 41.5 per cent showed hebephrenic features, affected sibs of catatonics to 60.9 per cent catatonic features and affected sibs of paranoids to 41.5 per cent paranoid features.

Kallmann [1938] who analysed his data with the so-called "Doppelfallmethode" (cf. *Schulz* [1936], p. 108) concluded more positively that the schizophrenic syndrome should be considered a genetic entity primarily caused by a single recessive major gene difference.

The methods used by *Schulz* and *Kallmann* must be considered rather inefficient for an analysis of the genetic homogeneity of schizophrenia. It would probably have been possible to sort out e.g. a type of schizophrenia caused by a major dominant gene with high penetrance if such a type had been mixed up with the material. It is, however, impossible to disprove the existence of several different recessive genes, or, if one counts with differences in penetrance, the existence of different dominant genes for schizophrenia. There would be endless possibilities to explain the fact that neither *Schulz* nor *Kallmann* found any positive evidence in favour of the genetic heterogeneity while it could still be there.

Slater [1947] found a strong positive correlation among siblings in regard to Kraepelinian picture. The P-value was < 0.001 for

catatonics and hebephrenics as well as paranoids. He also pointed out that *Kallmann* [1938] overlooked a likewise significant correlation in his data. These findings suggest that the disease picture at least partly depends on the major genic factor for schizophrenia and/or modifying genes.

A few studies which rather seem to favour the idea of the genetic heterogeneity should be mentioned to complete the picture. *Leonhard* [1942 and 1943], who belongs to the psychiatric school of *Kleist* in Germany, started his genetic studies with an entirely new and much debated subdivision of the schizophrenic syndrome. Whereas in his study with *Schulz* (*Schulz* and *Leonhard* [1940]) no significant differences regarding morbid risks for parents and sibs was found between *Leonhard*'s "atypical" and "typical" schizophrenias, there are some indications in this joint paper of a difference in this respect between his "typical" and "atypical catatonics". *Leonhard* [1943] reported a morbid risk for parents of catatonics of 3.3 ± 1.2 per cent and for siblings 11.1 ± 1.8 per cent whereas his "periodic" catatonics (included in the above figures) showed the corresponding risks of 5.4 ± 3.4 and 21.9 ± 5.2 per cent. *Leonhard* interpreted his findings in favour of dominant inheritance of his "periodic" type of catatonia. They cannot, however, be spoken of as more than suggestive.

Some more evidence pointing to a possible homogeneity of the *catatonic* subgroup (in the usual sense) was furnished by *Schwab* [1938] who examined 85 deteriorated catatonics and their parents and sibs. He found a morbid risk for parents of 5.5 ± 1.8 per cent and for sibs 8.2 ± 1.9 per cent. Among the affected sibs were 13 conclusive catatonics and a further 3 cases with questionable schizophrenic psychoses with catatonic features.

Another study which seems to favour the conception of genetic heterogeneity was conducted by *Weinberg* and *Lobstein* [1943] on a material of 199 *propositi* who were Dutch Jews. 27 of these came from first cousin marriages (13.6 per cent). Only 2.1 per cent of the parents were schizophrenics. With exclusion of those families where one parent was affected, the morbid risks of siblings from first cousin marriages were scored against siblings of not-related parents. A chi-square test of these data gave a P-value of between 0.01 and 0.02. The finding is suggestive of the existence of specific recessive genotypes or recessive modifiers.

The question of the genetic homogeneity of schizophrenia thus

has not been advanced very far since the first serious attempt to tackle it was made by *Schulz* some twenty years ago, although it would seem that there are more suggestions against this idea than for it.

As the situation appears at present it is only natural that the question of how this major genic factor for schizophrenia is inherited cannot be answered or if answered cannot be supported by sufficiently convincing data.

In general the tendency has been to abstain from too many speculations about the transmission of schizophrenia in a Mendelian sense and until more precise data have accumulated rest satisfied with empiric risk figures. Those who did express their views mostly seem to favour some type of recessive transmission.

Among the studies which concern large materials, only *Kallmann* [1938] made all the necessary subdivisions for a Mendelian analysis. Assumed that he were right in his hypothesis of the genetic uniformity of the schizophrenic syndrome, it would still seem impossible to accept his hypothesis of a single recessive gene difference without making extremely unlikely assumptions. It is, namely, as will be apparent from the following theoretical analysis, not possible to join his morbid risk of 0.85 per cent for the general population and 5.3 ± 0.5 per cent for parents with this latter hypothesis. Neither does it seem intelligible that the morbid risks for children were higher (16.4 ± 1.3 per cent) than for siblings (11.5 ± 0.7 per cent) or that the risk for siblings with one schizophrenic parent were only slightly higher and not significantly different from the risks for siblings with two non-schizophrenic parents ($1.96 \times \varepsilon$ [D]). In his papers of 1946 and 1950 which deal with new data, *Kallmann* reported a morbid risk of 9.3 ± 0.8 per cent for parents and 14.2 ± 0.8 per cent for siblings of schizophrenic twin index cases. In this connection it should be said as a general remark that *Kallmann* and most other workers in this field very often did not report the standard errors of their morbid risk figures. Then when cited in the literature also the number of observations to which they refer fall out and there remains a percentage which does not tell anything about the reliability. Practically all standard errors referring to quotations given in this paper were calculated by me. So, for instance, *Kallmann*'s [1938] risk figures for parents and siblings with two non-schizophrenic parents refer to relatively large data (the corrected rates of reference varying from 272 to 828) whereas the observations concerning the rest of the groups are rather small:

Siblings

(*Kallmann*, 1938).

Clinical group	One parent schizophrenic, the other not		Both parents schizophrenic	
	Corr. rate of reference	Morbid risk	Corr. rate of reference	Morbid risk
Hebephrenia . . .	101	14.9 ± 3.5	23.5	21.3 ± 8.6
Catatonia	79	17.7 ± 4.3	39.5	22.8 ± 6.8
Paranoid	61.5	9.8 ± 3.8	4.5	22.2 ± 22.2
Simplex.	43	16.3 ± 5.7	14	28.6 ± 12.5
Whole material . .	284.5	14.8 ± 2.1	81.5	23.3 ± 4.7

I have rearranged these figures so as to score schizophrenics against non-schizophrenics and under schizophrenics have been included also questionable cases. As *Kallmann* [1938] did not report the absolute figures for schizophrenic siblings, the collective morbid risks and their standard errors are estimates on the basis of the percentages given for the respective groups. It will be seen that none of the differences reach a value of three times its standard error. Thus a qualifying statement must be required on this point and the hypothetical procedure in introducing the percentages in the theoretical genetic calculations underlined.

The data reported by *Kallmann* [1938], as pointed out by *Koller* [1940], would fit the hypothesis of a major dominant gene difference better. On the recessive hypothesis, one would have to assume an assortative mating of 94 per cent which appears very unlikely. The extremely vague basis of *Kallmann*'s hypothesis is thus apparent. The argument that an increased incidence of consanguineous marriages (*Kallmann* [1946], p. 320) would support the recessive hypothesis is a misunderstanding. This would not be expected concerning a genetic trait occurring with a frequency as high as nearly one per cent.

Kallmann [1946] reported that of 211 schizophrenic twin index pairs without schizophrenia in their known ancestry, 5.7 per cent originated from unspecified consanguineous marriages. Of the remaining pairs, 95 had one schizophrenic parent and 283 had no schizophrenic parent, but schizophrenic cases in the collateral lines of ancestry. Information was inadequate about a further 102 pairs. No control data were given. The number of consanguineous marriages among all families about which adequate information was available does not appear particularly high unless they were all between first

cousins. Furthermore, it should be noted that in his extensive study of 1938, *Kallmann* did not find an increased consanguinity rate. It was rather lower as compared with the general population (*op. l.c.* p. 43).

Thus, if one tries to summarize the conclusions which are supported by facts, the following statements appear justified:

1. *The theory of the schizophrenic syndrome as being a genetic disease is scientifically sound.*

2. *It can be accepted as a scientific theory that the schizophrenic syndrome is caused primarily by major gene differences.*

To avoid misunderstandings, one should perhaps add above "the majority of the schizophrenic syndromes". It will, of course, not be denied that there are schizophrenic syndromes of non-genetic etiology but these apparently constitute an insignificant minority. The statements refer to what is important from a practical viewpoint.

COMMENTS ON THE PRESENT DATA OF SCHIZOPHRENIA

The more precise conclusions which were deemed justified have already been presented under the different headings of the detailed analysis of the data. There remains an attempt to join such conclusions which were considered reasonably correct into an intelligible picture and to examine how the view thus arrived at will compare with previous ideas. In this section, a number of hypotheses and suggestions will be advanced, many of them belonging to the category where I am perfectly willing to take some risk of being wrong. Much of the discussion will be based on the numerical values of the corrected morbid risk figures. It should be remembered that insofar as the different categories of relatives of the schizophrenic *propositi* are concerned, these figures have relatively large standard errors. They are therefore claimed to be nothing but reasonable estimates.

The approach of this study was different because the schizophrenics were selected from an isolate and therefore could be anticipated to represent a relatively homogeneous sample from a genetic viewpoint. However, it should also be true that the present population, including the schizophrenic families, is rather homogeneous from the environmental point of view. The observations made by me and my assistant during the field work did not offer anything in

support of the idea that the environmental conditions of the schizophrenic families would differ significantly from the average family of this area. Exceptions from this rule were observed only in some families, where the husband or the wife had contracted the disease but then poverty, conflicts and the like were always apparently a consequence of the disease and not a cause.

The concept of schizophrenia as caused primarily by a major gene difference has received further support through this study. The rather uniform clinical picture, interpreted as one with dominating catatonic features of a periodic tendency, could be explained either by genetic homogeneity or by similar environmental precipitating factors. It is well known that the symptomatology of schizophrenia is greatly influenced by the environmental experiences of the patient. Insofar as genetics is concerned, one does not necessarily have to think of the specific genic factor for schizophrenia but also of genetic modifiers such as those which are connected with the athletic or heavy muscular leptosome body type which was prevalent in the area.

The very high morbid risk of schizophrenia within the area (about 3 per cent) in connection with a risk for parents and siblings not exceeding that of other studies was interpreted as additional evidence in favour of the genetic theory. This is just the situation which could be anticipated on the basis of what we know about the behaviour of genetic traits subjected to genetic drift in isolates. The present rather high equilibrium would also be in agreement with the moderately expressed selection against schizophrenia in this area, the reproductive fitness being about 70 per cent.

From a psychoanalytic viewpoint it could, of course, be objected that in this area there would be more family units in which children became the victims of psycho-sexual traumata. However, until the advocators of purely environmental hypotheses have shown with equally precise methods of investigation that the morbid risk of schizophrenia increases with particular environmental circumstances in the absence of consanguinity, such explanations will remain speculations. Furthermore, it should be pointed out that most and probably all individuals who later on develop schizophrenia display a peculiar psychology years before the clinical onset of the disease. It would be equally correct to explain the conflicts which environmentalists regard as causes, as instead effects of these psychological peculiarities which by themselves might depend on the genetic factor for schizophrenia.

If we accept the theory of one main major genic factor being responsible for the development of the schizophrenic syndrome and the hypothesis that the schizophrenics of the present data represent a genetically homogeneous material, it remains to test in more detail the nature of this factor, i.e. how it behaves in a Mendelian sense. The difficulties involved in such an undertaking have already been stressed in previous paragraphs and it can only be a matter of by trial and error trying to arrive at the best estimate.

On the basis of certain premises, theoretical morbid expectancy figures have been calculated and were compared with the observed corrected risks. The most important assumption that must be made concerns the validity of the *Hardy-Weinberg* law within the isolate. This law is, of course, always an approximation and here it was considered valid only within reasonable limits.

Calculation of theoretical morbid expectancy figures.

Monohybrid recessivity. The frequency of the dominant gene (D) is noted by d and for the recessive gene (R) by r. By definition then $r + d = 1$ and assuming that the *Hardy-Weinberg* law is valid $d^2 + 2rd + r^2 = 1$. RR-genotypes, only, can develop the character but the manifestation of the character is assumed to occur in a certain number of RR-genotypes, only. This number is determined by the so-called penetrance, denoted p, i.e. p is the frequency with which RR-genotypes show phenotypical manifestation. Consequently, the population will consist of the following proportions of individuals

$$d^2 + pr^2 + (1—p)r^2 + 2rd = 1$$

The different types of individuals will be defined as follows:

Abbreviation	Characteristics genotypical	phenotypical	Frequency
DD	homozygote	normal	d^2
RR_m	homozygote	affected	pr^2
RR_o	homozygote	normal	$(1—p)r^2$
DR	heterozygote	normal	2rd

As by the selection of *propositi* we get access only to such families who are capable of producing affected offspring, we will examine on an *a priori* basis the distribution of affected and normal individuals in such families. The frequency of relevant matings and the expected frequencies of affected and normal children are shown in table 37.

Note that the sum under "mating frequency" will be the same as the sum of the frequencies of all children from these matings. By selecting individuals

Table 37. Calculation of theoretical morbid expectancy figures. Simple recessivity with the penetrance (p). Matings in which affected offspring occur. Further explanation in the text.

Type of mating	Mating frequency	Frequency of affected children (RR_m) in this type of mating
DR × DR	$4r^3d^2$	r^3d^2p
DR × RR_m	$4r^3dp$	$2r^3dp^2$
DR × RR_o	$4r^3d(1-p)$	$2r^3dp(1-p)$
RR_m × RR_m	r^4p^2	r^4p^3
RR_m × RR_o	$2r^4p(1-p)$	$2r^4p^2(1-p)$
RR_o × RR_o	$r^4(1-p)^2$	$r^4p(1-p)^2$
Total	$r^2(2-r)^2$	r^2p

(children) who are affected the frequency of affected individuals (k_1) among their parents can be calculated as

$$k_1 = \frac{pr}{2-r} \tag{1}$$

In families capable of producing affected offspring the incidence of affected children (k_2), irrespective of the parental combination will be

$$k_2 = \frac{p}{(2-r)^2} \tag{2}$$

As these families have to be selected in the usual way by affected *propositi*, it is better to formulate k_2 as being the incidence of affected among the siblings of *propositi*, irrespective of parental combination.

The further consequences, as formulated with a practical view-point in mind, are as follows. The incidence of affected siblings (k_3) of the *propositi* if both parents are normal is

$$k_3 = \frac{p(1-rp)^2}{[2-r(1+p)]^2} \tag{3}$$

The incidence of affected siblings of the *propositi* if one of the parents is affected is

$$k_4 = \frac{p(1-rp)}{2-r(1+p)} \tag{4}$$

The incidence of affected siblings of the *propositi* if both parents are affected is

$$k_5 = p \tag{5}$$

Finally the incidence of affected individuals (k_6) in the populations is by definition

$$k_6 = pr^2 \tag{6}$$

The simplest way to calculate the penetrance would, of course, be to examine matings of type $RR_m \times RR_m$ [equation (5)]. Considering, however, that genetic diseases generally appear with rather low incidences, these types of matings are too rare to be practically useful. I have chosen to combine and solve equations (1) and (6) in regard to p and r, but, of course, any two equations might be used depending on which k-values are known and the type of problem involved.

$$r = 1 - \sqrt{1 - \frac{k_6}{k_1}} \tag{7}$$

(as r can obviously not exceed unity)

$$p = \frac{k_6}{\left(1 - \sqrt{1 - \frac{k_6}{k_1}}\right)^2} \tag{8}$$

Monohybrid dominance. First we assume that the penetrance is the same for DD and DR. The different types of individuals and their frequencies are:

Abbreviation	Characteristics genotypical	phenotypical	Frequency
DD_m	homozygote	affected	d^2p
DD_o	homozygote	normal	$d^2(1-p)$
DR_m	heterozygote	affected	$2drp$
DR_o	heterozygote	normal	$2dr(1-p)$
RR	homozygote	normal	r^2

According to the *Hardy-Weinberg* formula we have:

$$d^2p + d^2(1-p) + 2drp + 2dr(1-p) + r^2 = 1$$

The different types of matings which can produce affected offspring and their frequences are given in table 38. From this table the different k-values can be calculated and the following formulae obtained.

$$k_1 = \frac{p}{d^2 - 2d + 2} \tag{9}$$

$$k_2 = \frac{p\ [3d(1-p) + 2]}{2d(1-2p) + 4} \tag{10}$$

$$k_4 = \frac{p\ [3d(1-p) + 1]}{d(3 - 4p) + 2} \tag{11}$$

$$k_5 = \frac{p(3 - 2d)}{(2 - d)^2} \tag{12}$$

$$k_6 = pd\ (2 - d) \tag{13}$$

At the calculation of formulae (10) and (11) matings occurring with frequencies containing the factor d^3 or d^4 have been omitted. The error is very small and of no importance in this connection.

Table 38. Calculation of theoretical morbid expectancy figures. Simple dominance with the penetrance (p). Matings in which affected offspring occur. Further explanation in the text.

Type of mating	Mating frequency	Frequency of affected children in this type of mating
$DD_m \times DD_m$	d^4p^3	d^4p^3
$DD_m \times DD_o$	$2d^4p(1-p)$	$2d^4p^2(1-p)$
$DD_m \times DR_m$	$4d^3rp^2$	$4d^3rp^3$
$DD_m \times DR_o$	$4d^3rp(1-p)$	$4d^3rp^2(1-p)$
$DD_m \times RR$	$2d^2r^2p$	$2d^2r^2p^2$
$DD_o \times DD_o$	$d^4(1-p)^2$	$d^4p(1-p)^2$
$DD_o \times DR_m$	$4d^3rp(1-p)$	$4d^3rp^2(1-p)$
$DD_o \times DR_o$	$4d^3r(1-p)^2$	$4d^3rp(1-p)^2$
$DD_o \times RR$	$2d^2r^2(1-p)$	$2d^2r^2p(1-p)$
$DR_m \times DR_m$	$4d^2r^2p^2$	$3d^2r^2p^3$
$DR_m \times DR_o$	$8d^2r^2p(1-p)$	$6d^2r^2p^2(1-p)$
$DR_m \times RR$	$4dr^3p$	$2dr^3p^3$
$DR_o \times DR_o$	$4d^2r^2(1-p)^2$	$3d^2r^2p(1-p)^2$
$DR_o \times RR$	$4dr^3(1-p)$	$2dr^3p(1-p)$
Total	$1-r^4$	

Solving equations (9) and (13) and thereby neglecting d^3 and d^4 as values of second and third order we obtain

$$d = \frac{1}{3} - \sqrt{\frac{1}{9} - \frac{k_6}{6k_1}} \qquad (14)$$

It is obvious that this formula (14) has no general validity. It is an approximation which can be used if the numerical value of d is small. The usefulness of the formula decreases with increasing numerical value of d on a gliding scale. It should, however, be safe to say that up to a d-value of 0.10 the approximation is sufficient for practical purposes, i.e. for tests we want to apply later.

Monohybrid dominance will now be considered under another assumption. In case of dominant genetic diseases and defects, these traits are generally so infrequent in the population that on good grounds one can assume that practically all affected are heterozygotes. However, in a few cases where probable homozygotes have occurred, these individuals seem to display the defect in a more severe form. Such individuals have so far been described only in relation to dominant defects with complete penetrance. It would, however, seem adequate to assume that the presence of a dominant gene in double dose would have another effect than when it occurs in single dose. Therefore we will examine the k-values under the assumption that DD-individuals show complete penetrance whereas DR-individuals have the penetrance p. We now have the following types of individuals.

Abbreviation	Characteristics genotypical	phenotypical	Frequency
DD	homozygote	affected	d^2
DR_m	heterozygote	affected	2drp
DR_o	heterozygote	normal	2dr(1—p)
RR	homozygote	normal	r^2

According to the *Hardy-Weinberg* formula we have

$$d^2 + 2drp + 2dr(1—p) + r^2 = 1$$

The different types of matings which can produce affected offspring and their frequencies are given in table 39. From this table the different k-values can be calculated and the following formulae obtained.

Table 39. Calculation of theoretical morbid expectancy figures. Simple dominance with the penetrance (p) for heterozygotes and complete penetrance for homozygotes. Matings in which affected offspring occur. Further explanation in the text.

Type of mating	Mating frequency	Affected children
DD × DD	d^4	d^4
DD × DR_m	$4d^3rp$	$2d^3rp + 2d^3rp^2$
DD × DR_o	$4d^3r(1—p)$	$2d^3r(1—p) + 2d^3rp(1—p)$
DD × RR	$2d^2r^2$	$2d^2r^2p$
DR_m × DR_m	$4d^2r^2p^2$	$d^2r^2p^2 + 2d^2r^2p^2$
DR_m × DR_o	$8d^2r^2p(1—p)$	$2d^2r^2p(1—p + 4d^2r^2p^2(1—p)$
DR_m × RR	$4dr^3p$	$2dr^3p^2$
DR_o × DR_o	$4d^2r^2(1—p)^2$	$d^2r^2(1—p)^2 + 2d^2r^2p(1—p)^2$
DR_o × RR	$4dr^3(1—p)$	$2dr^3p(1—p)$
Total	$1—r^4$	

$$k_1 = \frac{d + 2p(1 — d)}{4 — 6d + 4d^2 — d^2} \tag{15}$$

$$k_2 = \frac{p}{2} + \frac{d(1 — p)}{4(1 — dp)} \tag{16}$$

$$k_4 = \frac{(d + p)^2 — dp\ [2d(1 — p^2) + p(1 + d + 2p)]}{d(d + 1) + 2p + 4dp\ (dp — p — d)} \tag{17}$$

$$k_5 = \frac{(d + rp)\,[d + rp(1 + 2p)]}{(d + 2rp)^2} \tag{18}$$

$$k_6 = d^2 + 2dp(1 — d) \tag{19}$$

Solving equations (15) and (19) and thereby neglecting d^3 and d^4 as values of second and third order we obtain

$$d = \frac{1}{3} - \sqrt{\frac{1}{9} - \frac{k_6}{6k_1}} \qquad (20)$$

which is the same as formula (14). If with increasing d-value d^3 and d^4 cannot be neglected, the formulae will be different.

Genetic interpretations.

The observed corrected values of k_1 and k_6 were used to calculate the morbid expectancy figures k_3 and k_4. The result is shown in table 40. If the observed values of 0.12 for k_1 and 0.03 for k_6 are correct simple recessive transmission would be excluded as one obtains a penetrance of 1.67 which is absurd. The standard error of the observed k_1 (0.120 ± 0.027), however, is relatively large. The probability that the real value should be as low as 0.06 is only one per cent. Substituting this figure together with the unchanged k_6 (0.03), we get the theoretical values of 0.112 for k_3 and 0.201 for k_4 with a theoretical penetrance of 0.36 and gene frequency of 0.29 (= r). The observed k_3- and k_4-values both deviate in the same direction and are lower. The probability for agreement under this rather extreme assumption is about 5 per cent for k_3 and between 5 and 10 per cent for k_4. The combined probability that one still would have to count with simple recessive transmission thus is very small.

There is another possibility to test the recessive hypothesis. As

Table 40. A test of recessive and dominant transmission in schizophrenia. Morbid risks: k_6 parents, k_1 general population, k_3 siblings of two non-schizophrenic parents, k_4 siblings of one schizophrenic and one non-schizophrenic parent, k_5 siblings of two schizophrenic parents.

	k_1	k_6	k_3	k_4	k_5	p	Calculated frequency of pathologic gene
Observed figures . . .	0.12	0.03	0.09	0.12	(0.60)[1]		
Simple recessivity calculated	—[2]	—[2]	impossible			1.67	
Simple dominance with same penetrance for DD and DR, calculated	—	—	0.12	0.12	0.17	0.22	0.07 (= d)
Simple dominance with complete penetrance of DD only, calculated .	—	—	0.11	0.14	0.43	0.19	0.07 (= d)

[1] One family only.

[2] The calculated frequencies of k_3, k_4, k_5, p and gene frequency are based on the observed frequencies of k_1 and k_6.

mentioned, it would theoretically be compatible with a 5 per cent incidence among the parents, although it was unlikely. The probability of both a parent and one of his parents being affected would on this assumption be 0.25 per cent. In the total material there were 123 cases with a conclusive diagnosis. In three pedigrees a parent and one of his parents were schizophrenic. Concerning four of these six cases the diagnosis was not conclusive due to insufficient professional information. Referring to what has been said previously about the questionable diagnoses, however, this should diminish the value of the observations only slightly. The probability of such an occurrence, provided the diagnoses were definite, would only be 0.4 per cent (calculated on binomial distribution of rare events).

If one takes all observations into consideration, there appears to be no good reason to advance the hypothesis of a simple recessive gene difference.

The observations were best adapted to the hypothesis of a simple dominant gene difference (cf. table 40). Of the two alternatives shown in this table, the last one appears more likely as the morbid risk for siblings with two schizophrenic parents was higher than for the other two categories (cf. p. 67).

I therefore venture to advance the hypothesis that the type of schizophrenia prevalent in the investigation area is primarily due to a major simple dominant gene with a heterozygous penetrance of about 20 per cent and a homozygous penetrance of about 100 per cent. The frequency of this gene in the population was estimated at about 7 per cent. The penetrance refers to a schizophrenic psychosis as defined in this paper.

As a suggestion is offered the explanation that this major gene is concerned with some metabolic disorder which may express itself psychologically in the rather uniform schizophrenic syndrome with its characteristic periodicity where episodes of withdrawal and preoccupation change with episodes of excitement and physical hyperactivity.

It should be clearly understood that the explanations offered here concern exclusively the schizophrenic syndrome of the investigated area and the period of time covered by the study. It will remain an open question whether or not the observations have some general validity. If it will be possible to show with the same degree of probability that in another material the schizophrenic syndrome could be attributed to a recessive mode of transmission, fair evidence for its genetic heterogeneity would have been obtained.

The penetrance in this material, suggested at about 20 per cent, may seem very low especially as compared with the previously reported penetrance of some 85 per cent from twin data (*Kallmann* [1950]). Some explanations referring to diagnostic differences and the special genotypic constitution of monozygotic twins were mentioned on p. 77. It would seem apparent that penetrance values calculated on observations on monozygotic twins cannot without reservations be assumed to have general validity. One should observe, for instance, that the morbid risk of siblings of two schizophrenic or questionably schizophrenic parents has been calculated at 23.3 ± 4.7 per cent and of children of the same parental combination 63.4 ± 7.5 per cent (*Kallmann* [1938]) or 45.4 ± 6.3 per cent (*Schulz* [1940]). Even if the recessive hypothesis were correct, this would mean that the penetrance was appreciably influenced by genetic modifiers which in the case of monozygotic twins to a much greater extent would be selected together with the schizophrenic index case and his anticipated major recessive pair of genes.

Finally, the genetic theory of schizophrenia contains nothing of a nihilistic attitude. If a major genic factor is primarily responsible, it remains to find out how it works. The geneticist's duty is to advance this theory. This has been done by several independent investigators and the theory is based on adequate data, which by now are rather voluminous. Gene action belongs to the field of biochemistry and physiology and only through a collaboration between biochemists, physiologists and geneticists will it be possible to unravel the phenogenetics of this disease. Until we know what exactly the main "schizophrenic" gene does to the metabolism of the individual there can be no talk about specific therapy. Whoever thinks a genetic disease equals an incurable disease is quite mistaken. Pathologic gene action can be stopped as can the action of viruses or nutritional deficiences. Perhaps the schizophrenic cannot live a normal life without an unknown enzyme which has to be dispensed to him throughout his life. This might be because he is genetically different from other people. Nobody can live a normal life without continuously taking vitamins because we are genotypically so constructed that we cannot produce them within the organism. Thus if the genetic theory of schizophrenia is correct, what is the difference between the possible curability of schizophrenia in terms of biochemical genetics as compared to the curability of an avitaminosis?

CHAPTER III

MANIC-DEPRESSIVE PSYCHOSIS

Among all cases who had been hospitalized during the total period of 1902–1949, there were only eleven who had received the diagnosis manic-depressive psychosis. Concerning six of these cases, three of whom were living and resident on September 1, 1949, and a further three who had died prior to that date, the extended observation and the course of the disease left no reasonable doubt that they all represented true schizophrenic psychoses. Initially they had displayed a periodic course with episodes of excitement and agitation which often is extremely difficult to differentiate from periodic manic states. Subsequently, however, all the cardinal signs of schizophrenia according to the concept used in this paper became quite obvious.

Of the remaining five cases, I am inclined to regard only two as fairly typical manic-depressives. One, no. 88/49, did not belong to the present population but was merely resident in the area for 7 years. The other had migrated in 1946.

88/49 K. L., female born 1918. She was normally developed and had displayed no premorbid psychological oddities. Graduated as a public school teacher. Married 1942 and had three children 1943, 46 and 48. Material conditions were harmonious. During the last puerperium she suffered from insomnia and anxiety and contemplated over religious questions. Was afraid she might become insane. In February, 1949, she tried to commit suicide by taking phenobarbital. Taken to a hospital, she was deeply depressed and was afraid she might be punished for her attempted suicide but displayed no other psychotic symptoms. After a few days she ran away from the ward and made an attempt to jump into a river. After that she was transferred to the State Mental Hospital in Piteå. She was deeply depressed, cried, accused herself of various sins and said she wanted to die. She believed her children were dead. She displayed a rather pronounced psychomotor inhibition. At the hospital she was sometimes rather disturbing, cried and said she suffered from visual as well as auditory hallucinations. She made several attempts to strangle herself and once also tried to injure her eyes. Her physical examination was negative, her body type was described as leptosome. She received electroshock and insulin therapy. After a few relapses she recovered and was dismissed on supervision in August, 1949. *Hospital diagnosis:* manic-depressive psychosis.

233/49 S.V.K., female born 1920. She was sickly and nervous as a child. She graduated from an ordinary public school and later worked as house help. She was of average intelligence, a good worker and had a gay and optimistic mood. In 1937, she believed she had become pregnant and developed an acute confusional psychotic episode. She was taken to a gynecologic clinic where at first she was interpreted as a case of psychosis in pregnancy (intoxication). She was agitated,

incoherent, confused, showed stereotypic movements and laughed for no apparent reason. It was, however, disclosed that she was not pregnant. She recovered completely after a short time and said she did not remember much of her illness, "everything had been as in a fog". During the spring of 1938 she had a few short episodes of exaltation and elation. In 1939 she developed a typical acute manic episode and was now for the first time taken to a mental hospital. Physical examination was negative. Her body type was pycnic. She had no hallucinations or schizophrenic symptoms. She recovered completely and was dismissed after one month. In 1940 she had another similar episode and again spent a month at the mental hospital. From 1941 to 1946 when she migrated from the investigation area she stayed well. *Hospital diagnosis:* manic-depressive psychosis.

The following two cases are more difficult to interpret. It should be observed that both were "pure" Lapps. Even if the relatively few "pure" Lapps who belonged to the investigation area were sedentary, their way of life was in many respects different from that of the rest of the population. Furthermore, their constitution, temperament etc. is different. Such differences might well influence the symptomatology of their psychoses. Practically nothing is as yet known about the psychiatric morbidity among the Lapps.

190/49 A. S. K., female born 1879, died 1941. She was ordinarily gifted, sensitive and easily defeated. Without any known precipitating causes, she experienced six psychotic episodes between 1914 and 1920. These episodes were characterized by insomnia, exaggerated talkativeness and visual hallucinations of mostly religious content. Her stream of thought was largely incoherent. She also believed that people wanted to harm her. A few times she tore her clothes into pieces. Between the episodes she was said to have been quite normal. In 1920 she was admitted to the State Mental Hospital in Piteå. Physical examination was negative except that she was very thin (weight: 38 kg. height: 143 cm.). She was then dull, apathetic, disorientated temporally as well as spatially, but not confused. Her answers were mostly incoherent. She denied that she was nervous but complained about headaches and chest trouble. She denied hallucination but added, "one does not want to talk about everything one sees". At the hospital she alternated between restless overactivity and apathy. She improved gradually and was dismissed after 3 ½ months. About a year later, she experienced severe agitation and was again taken to the hospital. Day and night she had been walking around in the village gesticulating and talking loudly to herself. At the examination she was not confused. She answered at first rather adequately altough she had no idea where she was and called the doctor "the King of Sweden". Then she became more agitated and elated, started to talk unintelligibly, laughed, made faces and screamed. After about three months, she improved and was dismissed. She said she had not been able to master her thoughts, had heard voices and music and had seen people who appeared strange, as in a fog. In 1925 she was again readmitted. Since she came home she had been irritable and incoherent. At the exploration she was rather agitated and elated. She answered nonchalantly and controversially and repeated words or sentences. Said she heard voices "nasty as enemies". It was

"the old ones" who talked to her, she could also see them. In 1926 she improved and went back home. In 1940 she was again readmitted. During 1926–40 she had been more or less agitated all the time. She talked to persons who were not in the room and said she belonged to a very distinguished family. Sometimes she would cover herself with the bed-clothes and refuse to raise. She had visual and auditory hallucinations. Finally she became aggressive and threatened to burn the village. She explained the devil had given her orders to do so. She escaped through a window and went to several neighbouring farms and threatened to kill the people. She said she could see 4,500 years ahead. At the hospital she was mostly agitated, sang and screamed. Answered incoherently but was not confused. She did not improve and died in 1941 of a cardio-renal disease. *Hospital diagnosis:* manic-depressive psychosis (senile).

This patient displayed obvious schizophrenic components in her symptomatology. She was practically constantly ill for her last 20 years and the description indicates an increasing regression. A diagnosis of schizophrenia would have been more correct according to the writer's concept. A combination of schizophrenia and manic-depressive psychosis is, of course, possible. The exclusion of this patient from the previous analysis of schizophrenia, however, is immaterial. She would not have been accepted as conclusive, she died before the cross-section date and was not a sibling of a schizophrenic *propositus*.

263/49 M. P. K., female born 1876. As a young girl she was said to be of average intelligence, industrious, but rather odd. She married in 1911 and had four children, of whom the last one was born in 1918. Her husband died in 1929 and the patient then lived with one of her married sons. During 1919 to 1939 she experienced six psychotic episodes during which she was agitated, aggressive and violent so that the relatives had to watch her permanently. In March, 1939, she became confused and restless. She fell against the oven and contracted an appreciable head injury. The psychotic episode probably started before this accident but the information was not definite on this point. After the accident her condition became worse. She wanted to run away at nights, refused to take food, took off her clothes, was unclean, spit everywhere. She talked incoherently and was very irritable. At the end of the month the relatives could no longer handle her and she was taken to the State Mental Hospital in Piteå. Physical examination was negative except for some large hematomas on the face, arms and legs plus a blood pressure of 180/100. She was restless and disordered. Her mood was unstable, alternating between irritability and depression. Sometimes she cried. She answered mostly adequately but was disorientated spatially. Her memory was very poor. She displayed absolutely no insight concerning her psychic illness and denied all information given by the relatives and the local doctor. At the hospital she first refused to eat or take any medicine. Later she became somewhat more cooperative and started to work a little. All the time she adhered to the idea that she was at the hospital in Pajala. At examinations she was recusant and answered shortly. Nothing was wrong with her mind or thoughts, all she wanted was to get back

home to do her cooking. After three months she was able to take care of herself and her conduct was orderly. She was then dismissed. *Hospital diagnosis:* manic-depressive psychosis.

Postexamined by me in 1947: The now 73 year-old patient displayed an arterial hypertension of 220/150 and ophtalmoscopic signs of arteriosclerosis. According to the daughter-in-law who cared for the patient, she had not been able to do any work during the last some 15 years. Now she had to be helped with almost everything. Most of the time she was irritated and unclean. She was not able to give any coherent information about herself. The interview did not seem to interest her. She was disorientated temporally and spatially. In as much as she seemed to realize the questions, she answered haphazardly, controversially or unintelligibly. She displayed the picture of a pronounced deterioration.

This case, no doubt, appeared similar to no. 190/49 and the diagnosis must be questioned. However, not much information was available about the early psychotic episodes and the importance of the skull trauma is doubtful. The deteriorated stage in which she was found at the postexamination could very well be due to cerebral arteriosclerosis. Considering that she was 62 years old when her regression started, her condition could be interpreted as an involutional psychosis. It cannot, of course, be excluded that before that she had experienced a number of manic episodes. I have therefore changed the diagnosis to: manic-depressive psychosis ? + involutional psychosis (due to cerebral arteriosclerosis).

The fifth case was a retired nomadic Lapp who at 70 years of age experienced a short psychotic episode just before his death.

195/49 J.F.S., male born 1874. Died 1944 of bronchopneumonia and chronic myocarditis. He was a reindeer keeper who had been able to care for his family adequately. He had a stubborn, excitable and aggressive temperament. Since many years he had been a chronic alcohol addict. During 1941–44 he was admitted to the Home for the Aged in Pajala on three occasions and was treated for heart incompensation. Until 1941 he had been well and had had no mental illness. In 1944 he became confused and talked incoherently. Had outbreaks of rage and violence. At nights he ran around agitated and attacked the other patients at the Home. Said he had seen headless devils and lots of animals in his room. If they came back he would jump out of the window. He displayed no insight, said he was perfectly well, had a young wife and children all over the earth. He was taken to the State Mental Hospital where his agitation continued. He was negativistic, unclean and disordered. Mumbled and talked to himself. Slept only occasionally and sometimes refused to take food. Physical examination: pycnic body type. Myocarditis with heart incompensation. At the examination he was agitated, elated, could not concentrate and answered mostly incoherently. He seemed rather extroverted, praised himself and displayed some megalomanic ideas. Confirmed that he had visual hallucinations as mentioned above. His condition did not change and two weeks later he died. *Hospital diagnosis:* manic-depressive psychosis.

According to the diagnostic criteria used in this study, the above case could not be accepted under the diagnosis of manic-depressive psychosis. Even if the patient displayed a pycnic body type, the premorbid personality was probably not cycloid or cyclothymic. The etiologic significance of his alcoholism and chronic heart disease must be considered. The onset at the age of 70 does not favour the hospital diagnosis. The poverty of ideas, the incoherent talkativeness and the apparent deterioration also make the diagnosis doubtful. I therefore considered it justified to change the diagnosis to senile psychosis.

A further two cases had been diagnosed as questionable manic-depressives. As neither one fits the concept of this psychosis as outlined in this paper and they could not easily be referred to another diagnostic group, they will be reported.

23/49 K. E. K., male born 1908. He was healthy and displayed a normal development. Graduated from a public school. Had a small farm. He was an efficient worker and very thrifty. He was aggressive, easily excitable and very greedy. In 1948 he complained of persistent headaches, felt weak and tired. Had no appetite and lost weight. Subsequently he lost all initiative and stayed in bed. Could not sleep. In April, 1948 he was examined at the Division of Internal Medicine in Boden where his physical examination inclusive of skull x-ray studies was entirely negative. He then explained that since 1944 he had not been the same person as before. His wife said he had been irritable, wanted to be left alone, could not relax, went back and forth in his room and mumbled. The children were afraid of him. In January, 1949, he was admitted to the State Mental Hospital in Piteå at his own desire. His body type was athletic. His Wassermann reaction was negative. He had a slight tremor of his tongue, fingers and eyelids. Otherwise his physical examination was negative. At the exploration he was completely lucid, ordered and orientated. Somewhat tense and psychomotorically unstable. Felt sick. Complained mostly about weakness and anxiety. Denied hallucinations, ideas of reference and delusions. He improved somewhat after insulin in subcomatous doses and was dismissed in March, 1949. In June he was readmitted. Had not been able to sleep at home. His wife said he had threatened her and tried to abuse her and the children sexually, which the patient denied. At the hospital he walked back and forth in the corridor, mumbled and wanted to leave. Tried to steal the keys from the attendants.

Examined by me in September, 1949. Formally well kept personality. The intellectual contact was good. He was moderately depressed but could offer no explanation. Did not think he was particularly sinful or had done anything wrong. He was more apathetic and indifferent than emotionally engaged in his depression and there was nothing infectious about it. He complained about weakness and loss of initiative with a pronounced indifferent dryness. There was something distinctly unnatural about his general performance. He displayed a good insight. The whole picture was dominated by psychic tension, apathy and loss of affective modulation. No major psychotic signs were disclosed. However, I had the impression that the patient was not willing to talk freely about all his problems. He refused to discuss details about his behaviour at home.

As this individual was observed for a rather short time, only, and did not show conclusive symptoms, the diagnosis must be left open. However, there was strong reason to suspect an incipient schizophrenic psychosis. He was diagnosed as schizophrenia ?.

64/49 A. H. S., female born 1918. Normally developed. Graduated from a public school. Worked a couple of years as house help. Married 1936. Harmonious marital conditions. She had her third child in 1941. This child died by an accident shortly after birth. After that the patient became depressed, could not sleep. Said she was worthless and responsible for the death of her child. She was also very anxious because the child died before it had been baptized. In February, 1941, she was taken to the State Mental Hospital in Piteå. Thin, leptosome body type. Her thyroid was somewhat enlarged diffusely. Otherwise her physical examination was negative. She displayed a certain facial rigidity. She was somewhat confused and restrained. She answered slowly, often monosyllabic. There was a definite psychomotor retardation. She confirmed self-accusations. At the hospital she seemed somewhat perplexed and remained mildly depressed. She was able to attend to her personal needs. She received pentrozol shock treatment and improved rapidly. Two months later she was quite natural, gay and happy. She was dismissed under supervision of the Welfare Organization May 31, 1941, and finally discharged as completely recovered October 1, 1941. *Hospital diagnosis:* manic-depressive psychosis ?

She was postexamined by the superintendent of the Welfare Organization in August, 1946, when she displayed no psychopathologic symptoms. She had another 3 children and felt well and satisfied.

I would rather interpret this case as a choice between puerperal psychosis and reactive psychosis. The circumstances under which the child died no doubt constituted a psychic trauma which was strong enough to explain the reactions of the patient who then was not fully recovered after the delivery. I therefore preferred to change the diagnosis to reactive psychosis.

Insofar as manic-depressive psychosis is concerned, the findings thus were rather meager. Among all the individuals whom I had an opportunity to examine, not one single case was found who satisfied the diagnostic criteria to be placed in this clinical group. It is well known that manic-depressive psychoses are difficult to disclose in population studies of this kind especially if they occur as short hypomanic or moderately depressive episodes which may not completely prevent the individual from going on with his usual activities. When practising in the area, I encountered a few cases of moderately expressed depressions which were interpreted as belonging to the manic-depressive circle and occurring in individuals of cycloid temperament. However, in no case could one speak of a

psychosis. On the whole, people with cycloid temperaments were extremely rare whereas individuals with schizoid or schizothymic characteristics or a tendency to alternate between rather indifferent sluggishness or phlegmaticism and excitability to explosiveness were more common.

When stating that the manic-depressive psychosis was practically non-existent in the investigation area, it must be qualified that this statement refers to this disease as a major psychotic state. To what extent minor conditions belonging to this group might occur has not been investigated. Strictly there were two manic-depressives, of whom only one actually belonged to the present population, living and resident within the area on the cross-section date. Evaluated according to *Weinberg*'s abridged method with correction for a risk zone of 15–65 years, this would mean a morbid risk of 7 per 10,000, or, as both were females, 1.6 per 1,000.

CHAPTER IV
OTHER PSYCHOSES

The observations are too few to allow anything but a report of the number of cases disclosed and a very rough estimate of the morbid risk. All data refer to individuals living and resident in the investigation area on September 1, 1949.

There were three cases with *involutional psychosis*, one of whom also had the additional diagnosis of manic-depressive psychosis? (no. 263/49, see p. 95). Another of these cases, a woman 54 years of age, had been ill for about 5 years and carried a head injury in her history, making the diagnosis somewhat questionable.

Only one case of *senile psychosis* was found. The morbid risk of involutional and senile psychosis might be estimated by taking into consideration the population over 50 or 60 years of age. These figures were 0.33 and 0.61 per cent respectively. Compared with *Sjögren*'s [1948] population, the risks are about the same.

There were three cases of acute confusional psychoses which had occurred during pregnancy or puerperium. All had run a short course and ended with complete recovery. They were all postexamined by me, and I was unable to find any psychopathologic symptoms or abnormal character traits. The morbid risk of psychosis in relation to pregnancy or puerperium might be estimated roughly at 0.19 per

cent (calculated in relation to 50 per cent of the women between 15 and 45 years of age plus all over 45). The risks refer to major psychotic states requiring care in a mental institution.

Major alcoholic psychosis. Only one case was found, a man 49 years of age at the postexamination. He had been hospitalized under the diagnosis of *Korsakow*'s psychosis in 1940. Now at home, he was improved but somewhat deteriorated. A further two imbecile males had been hospitalized for one or two weeks on account of acute alcoholic delirium.

There was only one case of *reactive psychosis* which was described on p. 98 (no. 64/49).

Borderline cases. For the sake of completeness, it should be mentioned that a further two cases with so-called organic reaction syndromes were disclosed. They had displayed only slight and occasional psychotic symptomatology but were both moderately deteriorated and only partly able to take care of themselves. One man 49 years of age suffered from a pronounced *postencephalitic condition with Parkinsonism* (probably an *Economo*-encephalitis at the age of 19). The other case was a man 45 years of age who had been slowly deteriorating since the age of 24. His condition could be explained as a *post-contusional syndrome.* He had been examined by several internists and neurologists and encephalitis as well as multiple sclerosis had been discussed. However, during the last hospitalization his diagnosis was post-traumatic encephalopathy.

Finally, it should be mentioned that for the total period of time covered by this study, not one single case of *syphilitic psychosis* was disclosed. It is extremely unlikely that a case of general paresis would have been missed. Also other venereal diseases have occurred only very occasionally.

To be continued in vol. 4, no. 4 of this Journal. A summary and the bibliography will appear at the end of part II. A limited number of typewritten copies of the case histories will be made available for qualified research workers through the Swedish State Institute for Human Genetics, Uppsala, Sweden.

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NOTES ON DIFFERENTIATED POPULATION INCREMENTS

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In population statistics the usual practice when one wishes to know how much a population has grown in a year or in some other shorter or longer period of time is to deduct the figure for the beginning of the period from that representing the end of the period. The balance will denote the increase or, if negative, the decrease. Official statistics generally give annual population increases in terms of such figures. The same figures are occasionally obtained by other methods. The sum of deaths and emigrations is deducted from the sum of births and immigrations. The result will be exactly the same as when statistics of the whole population are used, provided available data are reliable.

It will be realized that such statistics, although they provide a rough idea of the position, cannot supply many details about the composition of the increments.

A population can at any given time be divided into individuals of three categories – children who are unable to work for their living and therefore have only potential economic value, adults who usually have high working ability and thus constitute a productively valuable group, and elderly persons with their working life behind them and a correspondingly scant influence on the national produce.

Evidently, therefore, simple numerical data are not enough when one wishes to establish whether a population has increased from one point of time to another. One must obtain supplementary evidence showing whether the increment is made up of children, of adults, or of old persons. An increment mostly composed of old people will be of doubtful value; one including a large proportion of children will range higher up the scale and will have a prospective value. An addition comprising persons of productive age will be best of all.

To get to grips with the problem we will take a hypothetical population. Let us isolate it from the outside world by prohibiting

emigration and immigration, and let us introduce compulsory contraception so no new babies will be born. If we compare the size of such a population on one occasion with its size on a subsequent occasion, it will obviously prove smaller on the last occasion. But the proportion of adults might none the less be greater. For the number of children who have become old enough to work can exceed the number of adults that have died or grown too old for productive work. Smaller number notwithstanding, the population has acquired a greater producing capacity, at least measured in terms of persons of productive age.

So far we have reckoned without the addition of individuals newborn during the period of observation. So let us make our compulsory contraception act a little less rigorously and allow comparatively few births, so that there still will be a net reduction of the population. Even then the productive capacity of our population could very well grow despite a numerical decline.

The above example was given to demonstrate theoretically that a numerical value for the growth of a population during a given period is too rough a measure to provide a focused picture of the situation. An extreme case, such as in our times scarcely could occur anywhere, was deliberately chosen for our example, the better to show that numerical population growths, such as are given in official statistics, provide a picture too undifferentiated for a proper assessment of the position. Information is needed about the nature of the increment – about the proportions of children, adults and old persons. In brief, an expression for the growth of a population must take into account the ages of the people that make up the increment. A purely quantitative estimate that makes no provision for individuals of different quality may be in order when a superficial survey is all that is required. On the other hand, if one is making a more discerning study of the situation, one must take the quality of individuals into consideration, particularly their age. If quality is disregarded, it would be the same as – and this is another extreme simile – if a man whose fortune was all in cash were to take stock of his financial status at the end of an accounting period on the basis of a count of coins and notes which paid no heed to their denominations.

We shall illustrate the need for differentiated data for population growth by citing some figures extracted from the official statistics for Sweden. Our examples will refer to one-year periods (1926–1948). This set of figures provide some idea of changes that have taken

place in a comparatively short period of time. Obviously the data for recent years given in our tables should not be compared directly with older figures.

In the first place we shall consider some changes that have occurred in various age groups, each of which comprises 5 years (cf. table 1).

This table and the corresponding table in an earlier paper (*Dahlberg* [1931][1]) reveal that at different times some age groups have grown from one occasion to another while others have decreased, and no very marked trend emerges from the figures. The changes seem rather unsystematic. While one age group has decreased, the next has grown bigger and the one after that has again decreased, and so on. In these circumstances the figures obtained will clearly be very dependent on the class limits used. Plainly an age group may have increased in spite of the fact that losses and gains have occurred in different sections of it. Thus an age group may have lost young individuals and gained old persons. The figures obtained represent the sum of the figures one would have obtained had the age groups been divided into smaller classes and the separate class figures added. It is impossible to determine how far one would have to carry this subdivision in order to get a true picture of the actual state of affairs. Consequently the table is not particularly interesting. It merely shows that the results of such an analysis are dependent on the class interval, and that the population as a whole may well have increased even if particular age groups have decreased markedly.

If all the individuals of a population were equivalent, the figures for the growth of the total population would of course represent an adequate measure of the situation. But the falseness of such an assumption is glaringly obvious. Conversely, it is of course difficult, not to say impossible, to find absolutely correct criteria for evaluation of separate individuals. Undoubtedly, however, whatever else they mean, the methods generally used today for designating a population increment imply an evaluation of individuals. All persons are considered equivalent; and the fallacy of such a presumption is so manifest that it should be worth while to device a better yardstick, even if it would be rash to hope for an absolutely correct evaluation of the separate individuals that constitute a «population increment».

It would seem best to value a person in terms of his contribution

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[1] *Dahlberg, G.*: Über Bevölkerungsvermehrung. Allg. statist. Arch. *21*, 276, 1931.

Table 1. Changes in the number of individuals in different age groups in Sw

Year	Age 0/5	5/10	10/15	15/20	20/25	25
1948	+ 2 605	+33 504	+11 846	— 8 962	— 9 811	+ 3
1947	+12 243	+27 585	+ 4 043	— 5 009	—11 524	+ 3
1946	+34 360	+ 3 919	— 4 032	— 5 935	—13 329	+17
1945	+39 015	+20 195	— 1 634	—11 005	—29 186	+13
1944	+37 970	+13 241	— 6 032	—13 332	— 2 007	— 6
1943	+32 041	+ 9 374	—10 633	—12 583	— 179	— 7
1942	+24 391	+ 1 484	— 6 213	—15 620	— 849	— 7
1941	+11 170	— 959	— 8 660	—21 654	+ 8 945	— 6
1940	+10 317	— 6 324	—10 606	—26 483	+18 212	— 5
1939	+13 059	— 6 491	—13 880	— 2 949	— 7 652	— 4
1938	+ 9 315	—10 884	—13 022	— 1 168	— 8 159	—
1937	+ 1 073	— 6 529	—16 273	— 1 133	— 7 727	+ 2
1936	— 1 219	— 8 975	—22 263	+ 8 057	— 6 924	+ 3
1935	— 7 145	—10 456	—26 768	+17 164	— 6 772	+ 6
1934	— 6 313	—14 325	— 3 586	— 8 224	— 4 805	+11
1933	—12 172	—13 356	+ 750	— 8 742	— 1 893	+ 9
1932	— 6 390	—15 793	— 1 583	— 8 128	+ 3 685	+ 7
1931	— 9 523	—22 124	+ 8 141	— 7 318	+ 4 117	+ 7
1930	—10 476	—26 039	+15 603	— 8 469	+ 5 574	+ 4
1929	—14 771	— 4 000	— 9 373	— 7 403	+ 8 381	+ 6
1928	—14 391	— 2 004	— 9 587	— 4 402	+ 6 582	+ 3
1927	—17 163	— 2 083	— 8 945	— 194	+ 3 389	+ 6
1926	—22 912	+ 8 244	— 8 348	+ 72	+ 3 233	+ 6

to the national product and use his working ability as the unit of measurement. That children cannot work is axiomatic, provided the age limit is not put too high. Similarly very old persons cannot work. Accordingly it would seem logical to have an age scale so constructed as to separate adults capable of working from children and old persons not so endowed: in a nutshell, keepers from the kept. Wherever drawn such limits would be arbitrary, but one should try to strike a balance providing a reasonable approximation of the true situation. I suggest that persons under 15 should be regarded as unable to or unsuitable for work, which on the whole would be correct. Obviously, however, youngsters slightly over 15 cannot be said to have a full working ability. So, if anything, this limit is set too low. As a corollary the upper boundary should also be set a bit too low, and the age of 60 seems a good compromise. As a rule some people not too much over 60 still retain a fair working ability, and it is often

ng the period 1926–1948 (+ indicates increase, — decrease).

	Age					Total
$/_{40}$	$^{40}/_{50}$	$^{50}/_{60}$	$^{60}/_{70}$	$^{70}/_{80}$	$^{80}/_{\omega}$	
924	+18 901	+13 809	+11 533	+10 628	— 1 042	+82 842
820	+18 500	+10 966	+ 9 749	+ 8 800	— 1 782	+79 361
473	+12 985	+20 776	+17 180	+ 6 771	+ 4 999	+89 936
073	+21 562	+ 5 202	+ 4 504	+ 5 050	— 1 941	+76 401
735	+16 189	+ 9 859	+11 519	+ 3 516	+ 2 378	+74 521
846	+14 066	+12 551	+ 9 220	+ 4 533	+ 3 789	+64 627
785	+17 445	+10 499	+10 269	+ 3 704	+ 3 951	+51 726
580	+16 730	+13 238	+ 8 914	+ 3 381	— 421	+35 042
452	+17 404	+10 284	+10 273	— 897	— 1 432	+30 129
673	+13 921	+ 6 914	+13 286	— 2 711	+ 1 401	+31 089
097	+12 458	+11 011	+13 835	— 5 472	+ 2 265	+25 492
625	+ 9 657	+ 9 651	+11 582	— 1 261	+ 364	+17 834
666	+10 350	+11 001	+ 4 959	+ 3 235	— 191	+16 382
926	+12 122	+ 9 963	+ 5 320	+ 2 957	+ 320	+17 416
692	+ 9 570	+12 637	+ 3 227	+ 3 694	+ 2 800	+21 524
317	+12 424	+ 9 879	+ 2 058	+ 6 000	+ 2 054	+21 202
143	+10 826	+10 947	+ 1 623	+ 5 500	+ 124	+27 918
542	+12 340	+ 8 472	+ 4 282	+ 675	— 1 026	+20 875
979	+ 9 518	+11 151	— 1 614	+ 5 010	+ 1 537	+21 491
412	+ 6 044	+14 876	— 3 449	+ 4 598	+ 1 668	+14 890
745	+10 875	+16 698	— 7 002	+ 5 577	+ 638	+17 267
704	+ 9 739	+13 043	+ 216	+ 2 674	— 1 603	+13 555
024	+10 918	+ 5 396	+ 7 038	+ 3 535	— 396	+20 806

quite considerable in those just beyond the limit. Here it is important to remember that sheer physical fitness is the decisive factor for the working ability of the majority of the population. Consequently one may safely assume that the working ability of most persons over 60 is significantly reduced. The limits we have drawn at the 15th and the 60th year of life are thus both certainly too low, and the intervening period may therefore be considered fairly correct[1]. The

[1] The Swedish Parliamentary Committee of 1928 on Pensions Insurance Scheme (Stat. off. utredn. 1930:15) issued a memorandum, in which the limits for productive life were drawn at ages 15 and 65, thus putting the upper boundary rather higher than I have done. However, in laying down the upper limit, the members of the committee were probably to some extent influenced by the statutory pensioning retirement age (age 67) and therefore put the limit rather high. That it tends to be too high seems particularly clear in view of the fact that the lower limit is put at 15 years, which makes 50 years the duration of productive life. Other things apart, however, the memorandum mentioned does prove that

objection might be raised that it would be realistic to put both limits somewhat higher. Quite so, and the length of the intervening period is of course to some extent arbitrary. Moreover, it should be kept in mind that persons between 15 and 60 apart from age are by no means equivalent. Some have a greater and some a smaller working ability. Others are for various reasons wholly incapable of working. And different education standards are also responsible for very marked variations in working ability (productive value). Those who are trained for various professions start their productive life late, in the mid-twenties or later. The proposed age classification is thus clearly very crude, yet there is no question that it is far superior to a method by which all persons in a population are considered equal[1].

Quite apart from the advantages outlined in the above, statistics in general would benefit from the universal adoption of this age classification and of the mathematical methods based on it. Only by so doing would internationally more comparable statistics be obtained. Of course, it would be fairly immaterial if my limits or other limits were agreed upon; the main thing is international uniformity.

The criticism of current methods of denoting population growth should not, of course, be taken to mean that data for what might be called numerical population increase are lacking in interest and ought to be done away with. Our aim has rather been to underline

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practical applications exist for an evaluation of individuals along the lines I have suggested. But the committee uses age as a yardstick only to determine the composition of the population – a method applied to the problem in question over and over again. As far as I am aware, however, it has not been utilized in studying population growth. At any rate official statistics have the disadvantage of consistently giving merely the numerical increments.

[1] A more correct estimate would result if one let working ability proportionately increase from zero at age 14 to unity at age 20, where it remained constant till age 65 when it began to fall off rectilinearly, reaching zero at about age 76. But even such a scale or something like it would in some respects be arbitrary and a schematization of the truth. For the sake of simplicity and convenience we have sacrificed in this publication a graded scale in favour of sharp age limits. Should it be desirable to study the matter in greater detail during a particular period of time, however, it would be necessary to make allowance for the fact that working ability only gradually attains adult level and in old age declines as gradually. Then the computations will not produce as marked changes from one year to the next. But one must also expect sharper discrepancies between the numerical and the «effective» population increases obtained.

the need for supplementary data when one requires a better comprehension of some aspects of the situation.

To realize the above objectives one must first of all determine what part of population increment falls in the group of children under 15 years of age, what part is composed of adults between 15 and 60, and what part is made up of old persons over 60. The respective figures for Sweden are given in table 2.

The table shows that the number of adult Swedes increased consistently from 1926 to 1948. I wish to mention that, with the exception of the period 1880–1890 in which the number of adults declined markedly, this has been the case since 1760 (cf. *Dahlberg* [1931]). The table also provides indirect information about a number of social trends. It is of course most interesting to study how the number of family supporters increases. The figure in question enables one to estimate the number of newcomers in production, and it also

Table 2. Changes in the number of individuals in the age groups 0–15 years, 15–60 years and above 60 years during the period 1926–1948.

Year	Age 0–15	15–60	60–ω
1948	+47 955	+13 768	+21 119
1947	+43 871	+17 723	+16 767
1946	+34 247	+26 739	+28 950
1945	+57 576	+11 212	+ 7 613
1944	+45 179	+11 929	+17 413
1943	+30 782	+16 303	+17 542
1942	+19 662	+14 140	+17 924
1941	+ 1 551	+21 617	+11 874
1940	— 6 613	+25 934	+10 808
1939	— 7 312	+26 425	+11 976
1938	—14 591	+29 455	+10 628
1937	—21 729	+28 878	+10 685
1936	—32 457	+40 836	+ 8 003
1935	—44 369	+53 188	+ 8 597
1934	—24 224	+36 027	+ 9 721
1933	—24 778	+35 868	+10 112
1932	—23 766	+44 437	+ 7 247
1931	—23 506	+40 450	+ 3 931
1930	—20 912	+37 470	+ 4 933
1929	—28 144	+40 217	+ 2 817
1928	—25 982	+44 036	— 787
1927	—28 191	+40 459	+ 1 287
1926	—23 016	+33 645	+10 177

indicates whether increased unemployment was due to a greater supply of labour or had other economical causes. The figure might therefore be named the productive population increment for short.

But the most important factor is the ratio between the number of family supporters and the number of supported. Simplifying, one might say that a population increment that leaves unaltered the relation between supporters and supported is immaterial, whereas it would have been beneficial had it tended to augment the relative number of supporters. To reason thus is not altogether correct. For a population increment will not be a negligible factor in the national economy even if it has no influence on the ratio between supporters and supported. The crux of the matter is whether the country's population density exceeds or falls short of the optimum level. If the country is sparsely populated an increment will be beneficial even if it has no effect on the ratio between supporters and supported. Conversely, such an increment would be undesirable if the country were too densely populated. At present, however, we hardly possess the means for exact determinations of a country's optimal population density. Nor can we estimate the significance of a population increment of the type under discussion when the density of the existing population lies at different levels above or below the theoretical optimum. In these circumstances the value of a population increment must be estimated without knowledge of either the level or the influence of the optimal population density. Yet, considering the economic structure of modern society, it would seem that a population increment that is not too large and does not influence the ratio between supporters and supported is of little significance today.

In this light we possess a mean of valuing the supported in terms of the supporters. All one has to do is to compute the ratio between supporters and supported at the beginning of the period studied. The effective population increment will then be the excess of productive persons over those required to keep the ratio unchanged. Conversely, the effective population increment will be obtained by estimating the number of productive persons required to restore the original state of equilibrium.

Thus the first thing to do is to find the ratio between supporters and supported. The best way is to compute the average number of supporters available for each person that must be supported. An *Index of Support* so calculated will be found in table 3.

The figures in table 3 are interesting in themselves. By the end

Table 3. Total number of individuals in the age groups 0–15 years and above 60 years and in the age group 15–60 years as well as index of support (i.e. the ratio between supporters and supported) during the period 1925–1948.

Year	0–15 + 60–ω	15–60	Index of support	Total population
1948	2 599 400	4 325 488	1.664	6 924 888
1947	2 530 326	4 311 720	1.704	6 842 046
1946	2 469 688	4 293 997	1.739	6 763 685
1945	2 406 491	4 267 258	1.773	6 673 749
1944	2 341 302	4 256 046	1.818	6 597 348
1943	2 278 710	4 244 117	1.863	6 522 827
1942	2 230 386	4 227 814	1.896	6 458 200
1941	2 192 800	4 213 674	1 922	6 406 474
1940	2 179 375	4 192 057	1.924	6 371 432
1939	2 175 180	4 166 123	1.915	6 341 303
1938	2 170 516	4 139 698	1.907	6 310 214
1937	2 174 479	4 110 243	1.890	6 284 722
1936	2 185 523	4 081 365	1.867	6 266 888
1935	2 209 977	4 040 529	1.828	6 250 506
1934	2 245 749	3 987 341	1.776	6 233 090
1933	2 260 252	3 951 314	1.748	6 211 566
1932	2 274 918	3 915 446	1.721	6 190 364
1931	2 291 437	3 871 009	1.689	6 162 446
1930	2 311 012	3 830 559	1.658	6 141 571
1929	2 326 991	3 793 089	1.630	6 120 080
1928	2 352 318	3 752 872	1.595	6 105 190
1927	2 379 087	3 708 836	1.559	6 087 923
1926	2 405 991	3 668 377	1.525	6 074 368
1925	2 418 830	3 634 732	1.503	6 053 562

of the eighteenth century (cf. *Dahlberg* [1931]) a degree of improvement seemed to have taken place, and in 1810 (as in 1780) there were 1 ½ supporters for each supported. Then, till the end of the nineteenth century, there was a somewhat jumpy impairment followed by a steady improvement with its peak in 1940. A clearer picture would of course be had by using one-year intervals throughout. But we have here refrained from such computations, as our sole objectives are to provide a general view and, chiefly, to motivate new methods of computation.

By virtue of the index of support in table 3 we can now proceed another step. We shall estimate the number of supporters required to take care of the accretion of supported included in the population increment for various periods. The quantity required is simply the

product of the number of supported and the supporting index. Obviously, in computing the net gain or loss of supporters the proper sign must be affixed to the supported expressed in terms of the number of supporters required to support them. Should the number of supported increase, the number of supporters they require must be subtracted from the number of supporters gained, if any, and vice versa. Thus the number of supporters gained or lost by the population is obtained. If positive, the theoretical figure indicates partly that the population as a whole has changed somewhat in numbers without altering the relation between supporters and supported, and partly that a given number of supporters have been gained. Should the theoretical figure be negative it means that the calculated number of supporters would have to be added to the adult population to restore the original ratio between supported and supporters. We call the quantity resulting from these operations the effective population change[1]. Such figures are given in table 4.

Table 4 moreover gives figures showing the numerical increase of the population. The eye is immediately struck by the very considerable discrepancy existing for most of the data shown in the table between the numerical and the effective population increment. Another remarkable thing is that the population may have increased considerably and yet show an effective decrease. For example between 1880 and 1890 the population increased numerically by more than 200000 persons while its effective decrease exceeded 300000 individuals (cf. *Dahlberg* [1931]). In 1890 about 300000 supporters would in other words have been required to restore the ratio obtaining in 1880 between supporters and supported. The reason for this is that in the decade in question the children increased

[1] The method of computing the effective population increase used here can mathematically be stated thus: Suppose the population at the outset comprises a supporters and b supported, and that during the period studied it receives an increment of d supporters and c supported. Let x be the number of persons constituting the effective population increase. Since, after deducting the effective increment from the supporters at the beginning and end of the period, we must have the same ratio between supporters and supported, the following is true:

$$\frac{a + d - x}{b + c} = \frac{a}{b}; \quad \therefore x = d - \frac{a}{b} \cdot c$$

If the supporters or supported have diminished in numbers, the sign prefixed to the quantities in question must of course be changed accordingly. If x is positive there is an effective population increase; if x is negative we are obviously dealing with an effective population decrease.

Table 4.

Year	Changes in the number of individuals in the age groups 0–15 years and 60–ω	Changes in the number of individuals in the age group 15–60 years	Calculated increase or decrease of the age group 15–60 years in relation to that of the age groups 0–15 and 60–w	Effective population change. Calculated gain or loss in the age group 15–60 years	Numerical population change	Total population at the end of the year
1948	+69 074	+13 768	—117 702	—103 934	82 842	6 924 888
1947	+60 638	+17 723	—105 449	— 87 726	78 361	6 842 046
1946	+63 197	+26 739	—112 048	— 85 309	89 936	6 763 685
1945	+65 189	+11 212	—118 514	—107 302	76 401	6 673 749
1944	+62 592	+11 929	—116 609	—104 680	74 521	6 597 348
1943	+48 324	+16 303	— 91 622	— 75 319	64 627	6 522 827
1942	+37 586	+14 140	— 72 240	— 58 100	51 726	6 458 200
1941	+13 425	+21 617	— 25 830	— 4 213	35 042	6 406 474
1940	+ 4 195	+25 934	— 8 033	+ 17 901	30 129	6 371 432
1939	+ 4 664	+26 425	— 8 894	+ 17 531	31 089	6 341 303
1938	— 3 963	+29 455	+ 7 490	+ 36 945	25 492	6 310 214
1937	—11 044	+28 878	+ 20 619	+ 49 497	17 834	6 284 722
1936	—24 454	+40 836	+ 44 702	+ 85 538	16 382	6 266 888
1935	—35 772	+53 188	+ 63 531	+116 719	17 416	6 250 506
1934	—14 503	+36 027	+ 25 351	+ 61 378	21 524	6 233 090
1933	—14 666	+35 868	+ 25 240	+ 61 108	21 202	6 211 566
1932	—16 519	+44 437	+ 27 901	+ 72 338	27 918	6 190 364
1931	—19 575	+40 450	+ 32 455	+ 72 905	20 875	6 162 446
1930	—15 979	+37 470	+ 26 046	+ 63 516	21 491	6 141 571
1929	—25 327	+40 217	+ 40 397	+ 80 614	14 890	6 120 080
1928	—26 769	+44 036	+ 41 733	+ 85 769	17 267	6 105 190
1927	—26 904	+40 459	+ 41 029	+ 81 488	13 555	6 087 923
1926	—12 839	+33 645	+ 19 297	+ 52 942	20 806	6 074 368

markedly and the old persons moderately at the same time as there was a fairly negligible diminution of the number of persons between 15 and 60 years old. The figure for the effective population decrease provides a simpler and more concrete expression for the change that took place. The numerical population increment is interesting mainly because it enables one to judge whether the population has a tendency to increase. Information about the productive population increase (or the productive population swing) is more useful when one wishes to estimate changes in available labour. The effective population increment (or the effective population swing), lastly, is significant in connection with prognostications regarding the influence of changes in population structure on the average standard of living.

Table 5 gives data for annual numerical, productive and effective population swings in Sweden during recent years expressed in units per thousand of the initial population. These figures too display marked discrepancies.

It may seem strange that computations of the numerical and of the effective change in population give results that can differ so markedly. The large discrepancies we have obtained would seem adequately to justify the methods advocated here. A knowledge of the excess of births over deaths simply is not enough, although it happens to be the common definition of population growth (when emigration and immigration can be neglected). One might say that this definition leads one to believe that the excess is composed of newborns. Even though a professional statistician would not be guilty of such a mistake, the definition does no doubt lead numerous students of population statistics astray. In addition the numerical

Table 5. The annual numerical, productive and effective population changes expressed in units per thousand of the initial population.

Year	Numerical population increase ‰	Productive population increase ‰	Effective population increase ‰
1948	+ 12.11	+ 2.01	— 1.52
1947	+ 11.59	+ 2.62	— 12.97
1946	+ 13.48	+ 4.01	— 12.78
1945	+ 11.58	+ 1.70	— 1.63
1944	+ 11.42	+ 1.83	— 1.60
1943	+ 10.00	+ 2.52	— 11.66
1942	+ 8.07	+ 2.21	— 9.07
1941	+ 5.50	+ 3.39	— 6.61
1940	+ 4.75	+ 4.09	+ 2.82
1939	+ 4.93	+ 4.19	+ 2.78
1938	+ 4.06	+ 4.69	+ 5.88
1937	+ 2.85	+ 4.61	+ 7.90
1936	+ 2.62	+ 6.53	+ 13.68
1935	+ 2.79	+ 8.53	+ 1.87
1934	+ 3.47	+ 5.80	+ 9.88
1933	+ 3.43	+ 5.79	+ 9.87
1932	+ 4.53	+ 7.21	+ 11.74
1931	+ 3.40	+ 6.59	+ 11.87
1930	+ 3.51	+ 6.12	+ 10.38
1929	+ 2.44	+ 6.59	+ 13.20
1928	+ 2.84	+ 7.23	+ 14.09
1927	+ 2.23	+ 6.66	+ 13.42
1926	+ 3.44	+ 5.56	+ 8.75

population increment gives even a professional statistician only casual information about population events.

We know that statistic comparisons of populations in general must take into account the age morphology of the populations compared. On the other hand, when it is a matter of population changes taking place over a year in one and the same population, one seems to assume more or less consciously that no marked changes can have occurred in the age morphology. This factor has therefore been neglected, and undifferentiated figures for the numerical population increment have been regarded as adequate. But small changes in the age structure of populations do nevertheless sometimes occur in the short space of a year. Over longer periods these small fluctuations add up to the sharp changes in the age morphology that are considered significant by statisticians. In fact, however, even small changes in the age morphology of an entire population presuppose a fairly sharp swing in the character of the increment responsible for the change. A crude analogy can be used to express the position thus: If one wishes to alter the composition of the water in a lake by adding annually a relatively small amount of other fluid, then the addition must, if its amount is insignificant, have some effect on the lake water; the added fluid must in other words be very different from the lake water. In this case it must be of interest to analyse not only the fluid added but also the water in the lake.

We shall now briefly discuss differentiation of population increments on a fertility basis. In these circumstances it is best to confine the computations to women. Women can be classified as fertile or barren. We might thus say that women between 18 and 45 years are fertile while those above and below these limits are infertile. Sweden's population in recent years is thus classified in table 6. It clearly reveals that the potential number of fertile women varies far more than the numerical population increase. We can of course express those who are potentially fertile in per cent of the population, but there is no necessity for relating the potentially fertile to the infertile. One can also carry the thing a step further by calculating by Kusinsky's or some other method the expected fertility. But the methods mentioned are enough to give an idea of the situation, although the impression obtained may be rough and can be variously improved. One method would be to weigh the women with respect to the fertility they displayed during the preceding period. But the latter procedure is unreliable and we have not used it.

Table 6. Annual changes in the number of women in the age groups 0–18, 18–45 and above 45 years during the period 1926–1948 and these figures in per cent of the total numerical increase or decrease of women during the same period.

Year	Increase Total number of women	Age groups 0–18 Number	0–18 Per cent of total number	18–45 Number	18–45 Per cent of total number	45–ω Number	45–ω Per cent total num
1948	+40 772	+20 394	+ 50.0	— 613	— 1.5	+20 991	+ 51.
1947	+37 478	+20 277	+ 54.1	— 1 765	— 4.7	+18 966	+ 50.
1946	+45 793	+16 417	+ 35.9	— 2 506	— 5.5	+31 882	+ 69.
1945	+33 573	+24 128	+ 71.9	+ 1 454	+ 4.3	+ 7 991	+ 23.
1944	+35 429	+18 105	+ 51.1	+ 333	+ 0.9	+16 991	+ 48.
1943	+31 752	+11 499	+ 36.2	+ 693	+ 2.2	+19 560	+ 61.
1942	+24 505	+ 4 323	+ 17.6	+ 1 565	+ 6.4	+18 617	+ 76.
1941	+14 635	— 3 900	— 26.6	+ 3 933	+ 26.9	+14 602	+ 99.
1940	+12 357	— 7 526	— 60.9	+ 5 531	+ 44.8	+14 352	+116.
1939	+13 733	—11 086	— 80.7	+11 814	+ 86.0	+13 005	+ 94.
1938	+11 748	—17 500	—149.0	+15 949	+135.8	+13 299	+113.
1937	+ 7 112	— 9 371	—131.8	+ 6 001	+ 84.4	+10 482	+147.
1936	+ 6 299	—10 515	—166.9	+ 5 480	+ 87.0	+11 334	+179.
1935	+ 6 655	—13 319	—200.1	+ 9 388	+141.1	+10 586	+159.
1934	+ 8 722	—13 616	—156.1	+ 9 866	+113.1	+12 472	+143.
1933	+ 7 842	—13 210	—168.5	+ 7 974	+101.7	+13 078	+166.
1932	+11 454	—14 022	—122.4	+11 763	+102.7	+13 713	+119.
1931	+ 7 276	—14 399	—197.9	+12 179	+167.4	+ 9 496	+130.
1930	+ 5 972	—14 526	—243.2	+10 715	+179.4	+ 9 783	+163.
1929	+ 6 506	—15 214	—233.8	+11 155	+171.5	+10 565	+162.
1928	+ 7 910	—13 840	—175.0	+12 309	+155.6	+ 9 441	+119.
1927	+ 5 975	—15 744	—263.5	+14 905	+249.5	+ 6 814	+114.
1926	+10 735	—12 380	—115.3	+14 289	+133.1	+ 8 826	+ 82.

Summary.

In these circumstances it seems justified to say that it would be desirable to have more differentiated data on population increments in the official statistics. The data ordinarily given would of course always be required, but it would be useful if the following type of information could be supplied as well.

1. Specifications of the increase or decrease, as the case may be, of the age class 0–15 years (= improductive population change), the age class over 60 years (= improductive population change) and the age class 15–60 years (= productive population change).

2. Data concerning the numbers of those between 15 and 60 years old, as well as of those below or above these limits at the end

of the base year, and a supporting index computed on the basis of these figures indicating the number of supporters (persons between 15 and 60 years old) required to support the rest (those below 15 and over 60 years old).

3. Information as to the theoretical effective population. Such a figure tells us a) the number of supporters required to restore the original balance obtaining at the point of time used as the starting point, b) any excess of supporters over and above those required to support the improductive portion of the population. In the first instance the theoretical figure will be negative, otherwise positive.

4. Finally, it would be interesting to have not only the numerical but also the improductive, the productive and the effective population swings in thousands of the population.

5. For women a division may also be used in regard to fertility, so that one gives the number of women below 18 and above 45 as infertile and the women between 18 and 45 as fertile. This will give a somewhat more correct expression of the situation from the point of view of fertility.

Résumé.

Dans les circonstances actuelles, il semble justifié de dire qu'il serait désirable de posséder des données différenciées sur les augmentations de population consignées dans les statistiques actuelles. Les informations habituelles seraient naturellement aussi nécessaires, mais il serait utile qu'elles fussent complétées par les renseignements suivants:

1. Spécifier si l'augmentation ou la diminution, selon les cas, concerne la classe de 0 à 15 ans (modification d'une population improductive), la classe au-dessus de 60 ans (modification d'une population improductive) ou celle comprise entre 15 et 60 ans (modification d'une population productive).

2. Données concernant le nombre des individus compris entre 15 et 60 ans ainsi que de ceux au-dessous de ces limites à la fin de l'année de base; sur ces indications, établir un index indiquant le nombre d'individus actifs (personnes entre 15 et 60 ans) nécessaires pour assumer la charge des autres (ceux au-dessous de 15 et au-dessus de 60 ans).

3. Indiquer le pourcentage d'individus actifs théoriquement nécessaire pour supporter la charge de la population non active. Ce chiffre indique soit a) le nombre d'individus actifs nécessaires pour

rétablir l'équilibre original au moment choisi comme point de départ de l'analyse soit b) un éventuel excès d'individus actifs, plus nombreux que ne le demanderait théoriquement la charge de la population non active. Dans le premier cas, le nombre «théorique» sera négatif, dans le second positif.

4. Enfin il serait intéressant d'avoir non seulement des données numériques, mais aussi les variations des populations effective, productrice, et non productrice en pour mille de la population.

5. On pourrait utilement établir également une classification des femmes par rapport à leur capacité de procréation: on indiquerait dans un groupe le nombre des femmes au-dessous de 18 ans et au-dessus de 45 ans, dans le groupe opposé celles entre 18 et 45 ans. On obtiendrait ainsi une expression relativement plus exacte des possibilités d'une population au point de vue procréation.

Zusammenfassung.

Der Verfasser betont, daß es begründet sei, differenziertere Angaben hinsichtlich der Bevölkerungszunahme in der offiziellen Statistik als wünschenswert zu bezeichnen. Natürlich sei der Erhalt derjenigen Ziffern für den numerischen Volkszuwachs, welche gewöhnlicherweise angegeben werden, zur Voraussetzung gemacht, doch wäre es von Wert, wenn außerdem noch folgende Angaben gemacht werden könnten:

1. Angaben bezüglich der Zunahme bzw. der Abnahme der Altersklassen 0–15 Jahre und über 60 Jahre (= improduktive Volksveränderung) sowie betreffs der Altersklassen 15–60 Jahre (= produktive Volksveränderung).

2. Angaben bezüglich der Anzahl Individuen zwischen dem 15. und 60. Lebensjahre sowie über die Anzahl übriger Individuen der Bevölkerung am Ende des Jahres, welches man bei der Berechnung der Volksvermehrung als Ausgangspunkt benutzt, samt einem auf der Grundlage dieser Ziffern errechneten Versorgungsindex, welcher angibt, wie viele Versorger (Personen zwischen dem 15. und 60. Lebensjahre) einem zu Versorgendem (d.h. einem Individuum im Alter von 0–15 Jahren oder über 60 Jahre) entsprechen.

3. Eine Angabe betreffs der berechneten, effektiven Bevölkerungsveränderung. Diese Ziffer gibt Aufschluß über: a) die Anzahl Versorger, welche erforderlich sind, um das ursprüngliche Gleichgewicht wieder herzustellen, welches in dem Augenblick bestand, wel-

cher den Ausgangspunkt zu dem Vergleiche bildet; b) den eventuellen Überschuß an Versorgern, welchen man erhält, nachdem man die Anzahl von Versorgern, welche erforderlich ist, um das ursprüngliche Gleichgewicht herzustellen, abgezogen hat. Im ersteren Fall erhält die berechnete Ziffer negatives, im letzteren positives Vorzeichen.

4. Endlich ist es von Interesse, Promillezahlen nicht nur für die numerische, sondern auch für die improduktive, die produktive und die effektive Bevölkerungsveränderung zu erhalten.

5. Für Frauen kann auch eine Einteilung im Hinblick auf die Fruchtbarkeit vorgenommen werden, dergestalt, daß man Frauen unter dem 18. und über dem 45. Lebensjahre als unfruchtbar und Frauen zwischen dem 18. und 45. Lebensjahre als fruchtbar betrachtet. Auf diese Weise erhält man einen korrekteren Ausdruck für die Situation vom Gesichtspunkte der Fruchtbarkeit.

Romanus T.: Acta genet., *4*, 117–123, 1953

From: The State Institute of Human Genetics, Uppsala, Sweden
(Head: Professor Gunnar Dahlberg, M.D., LL.D.)

INTEROCULAR-BIORBITAL INDEX. A GAUGE OF OCULAR HYPERTELORISM

By TORSTEN ROMANUS

The anthropology of the face remains to this day a territory more uncharted than one would expect. Probably the difficulty of finding convenient points to measure from, is a major reason for this defect. A new such point – first used in a preceding study on the so-called nose-lip index (*Romanus* [1952]) – is the inner canthus of the eye which has an important part in the present study as well.

A distinguishing characteristic of the face is the ratio of the distance between the eyes to the external biorbital diameter, i.e. to the width of the face at the same level. The latter expression will henceforth be avoided in order to preclude confusion with the

more commonly used "maximum bizygomatic diameter" (*Hrdlicka* [1920]). For the purposes of the present study the distance between the eyes was measured between the two inner canthi and the external biorbital diameter between the most lateral points on the temporal margin of the orbital wall. Let the two first points be called OO and the two latter LL, then the interocular-biorbital index can be expressed thus:

$$\frac{\text{LL in cm}}{\text{OO in cm}} \times 100; \text{ or}$$

$$100 \times \frac{\text{the maximal bitemporal diameter across the orbits in cm.}}{\text{the diameter joining the inner canthi of the eye in cm.}}$$

A loose but convenient name for this index would be eyespan-eyespace index.

These measurements are best taken by an observer sitting with his eyes at the same level as the subject's. After palpating the points of reference on the lateral orbital walls, the distance between them is conveniently taken with a pair of spreading compasses. The distance between the two nasal canthi can be estimated closely enough by sighting along the vertical jaws of sliding compasses till they bear on the canthi.

Theoretically the eyespan-eyespace index can vary from 100 to infinity. An index of 100 denotes that the distance between the eyes coincides with the width of the face in that plane. The eyes must in other words be situated on the lateral sides of the head. Several animals may be taken as examples. But if the index takes the value of infinity the distance between the eyes must be zero – the cyclops. In man the index will tend towards 100 times the centimetric width of the face, the shorter the distance between the eyes.

The eyespan-eyespace indices given in table 1 were recorded in a series of 144 Swedish students of medicine or dentistry.

The average adult will have an external biorbital diameter of about 12 cm. and an intercanthic diameter of about 3 cm., the latter thus having about one quarter of the width of the face at eye level. This means that most persons will have an index somewhere in the neighbourhood of 400. In pronounced ocular hypertelorism it will fall between 200 and 300. Case I in *Greig*'s [1924] pioneer work on ocular hypertelorism (fig. 5 in Greig's publication) had an eyespan-eyespace index of close to 200. In extreme *hypo*-telorism it will be as high as 900 to 1000.

Index	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	Total
330	1															1
340		1		4	1											6
350		1	2	1		1	1	1								7
360		2	4	3		1		1								11
370		3	5	2	1				1					1		13
380		3	1	2		2			1			1			1	11
390		1	3	1		2		1	1							9
400		6	3	5	4	1		1			1					21
410		2	1		2	1	1									7
420			1	1				1								3
430		2		1												3
440			2													2
450								1								1
460																—
470																—
480		1			1											2
380 [1] (382)	1	23	22	32	9	8	3	8	3		1	1		1	1	102

[1] Average. Mean and standard error of the mean: 382.5±3.1. Standard deviation and standard error: 30.9±2.2.

Women.

Index	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	Total
320		1		1												2
330	1	1														2
340			1	1												2
350	1				1	1						1				3
360	1	1	2	1												6
370	1	1	3	1		2										8
380	2		4		2	1	1									10
390		5	2		1		1	1		1			1			11
400		2	3	2	2	1						1				11
410	1	1	2		1		1									7
420		2	1		1											4
430	1	2	1	1												5
440																—
450																—
460																—
470			1													1
390 [1] (386)	8	16	20	7	8	5	3	1		1		2	1			72

[1] Average. Mean and standard error of the mean: 386.3±3.3. Standard deviation and standard error: 28.1±2.3.

The small difference between the mean eye distance indices for men and for women is not significant in the present study.

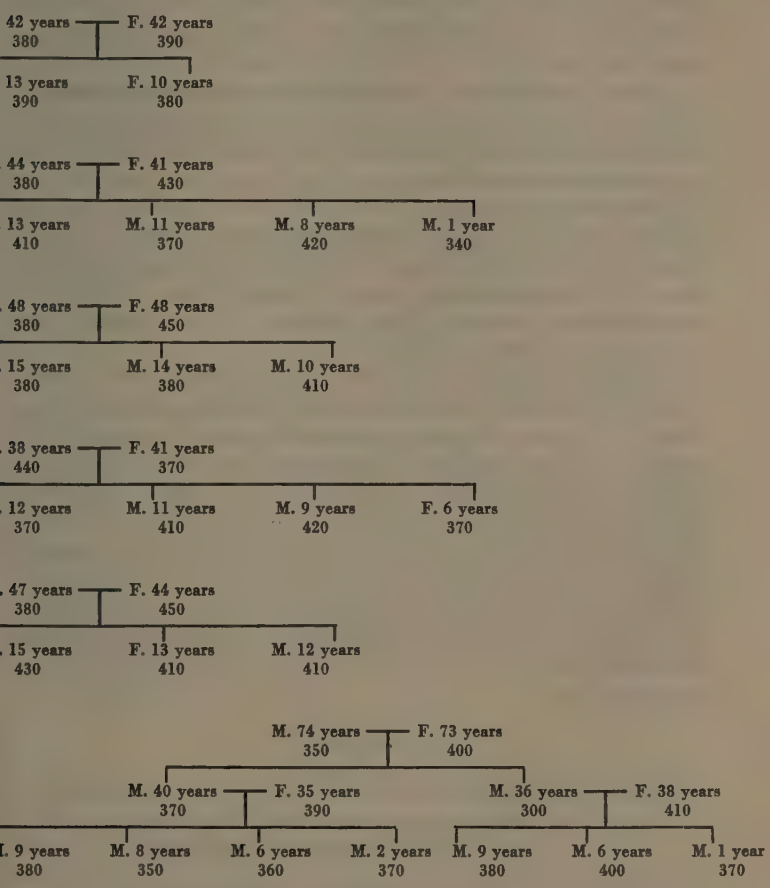
The length of the two diameters involved is of course mainly a manifestation of the structure of the skull. To get some idea of the degree of agreement between indices for skulls and indices for living subjects a series of skulls was measured. (The author is indebted to Professor *David Holmdahl*, Director of the Institute of Anatomy, Uppsala, for his courtesy in making skulls available.) Selected reference points: the most lateral points on the lateral orbital walls, representing the maximal biorbital diameter; and those points on the medial orbital walls situated the shortest distance apart, to correspond with the intercanthic distance.

Table 2 gives results recorded in a series of 215 skulls.

Table 2.

Eyespan-eyespace index	Number of skulls	Number of persons
320	1	4
330	—	6
340	—	8
350	1	10
360	3	17
370	4	21
380	16	21
390	17	20
400	21	32
410	19	14
420	32	7
430	16	8
440	26	1
450	14	—
460	21	—
470	9	1
480	5	2
490	5	—
500	1	—
510	—	—
520	1	—
530	2	—
540	—	—
550	1	—
	Total 215	174
	Axerage index 430	380
	(426)	(384)

Fig. 1. Interocular-Biorbital Index in some Families.



Even though these series – that of students and that of skulls – are not comparable strictly speaking, one would *a priori* expect somewhat higher figures for skulls. On skulls the shortest diameter between the medial orbital walls is shorter than the distance be-

tween the inner canthi, and therein lies the main reason for the difference. On the other hand, at least in Europeans, the soft tissue padding over the lateral orbital walls is thin and hence produces a relatively less important discrepancy between the biorbital diameter measured on skulls and that measured on living subjects.

Greig's monograph on ocular hypertelorism carries an illustration of the skull in case I. As measured on the skull the eyespan-eyespace index becomes something near 250, as against 200 or so when measured on the face.

The average eyespan-eyespace index varies from one race to another. Thus it is low in mongols, obviously due largely to their epicanthus which lengthens the distance between the eyes. Curiously enough the eyespan-eyespace index in portraits from the 18th and 19th centuries is frequently low, probably it is a reflection of the ideals of beauty then obtaining.

Hypertelorism has been reported as an accompaniment to mental disturbances and dysostosis cranio-facialis (see for example *Gates* [1946]). In these cases hypertelorism has been hereditary and transmitted by a dominant gene. Hypertelorism has also been seen in mentally intact subjects (*Allen* [1926]). Extreme values of the eyespan-eyespace index might prove of significance in cases of disputed paternity. To find out, the eyespan-eyespace index was measured in some families selected at random among friends and acquaintances, but no extremes were observed. In the human embryo, however, the optic primordia are oriented laterally, so one must expect a somewhat lower index in neonates where ossification of the skull is not complete.

Summary.

Interocular-biorbital index, i.e.

$$100 \times \frac{\text{the maximal bitemporal diameter across the orbits in cm.}}{\text{the diameter joining the inner canthi of the eye in cm.}}$$

varies theoretically from 100 to infinity. In pronounced ocular hypertelorism the index is about 200. Measured on soft parts the index in man falls between 380 and 390 on an average. Measured on skulls the index is somewhat higher or 430. Extreme values of the eyespan-eyespace index might prove of significance in cases of disputed paternity, since hypertelorism seems to be due to a dominant autosomal gene.

Résumé.

L'index interoculaire-biorbiculaire, c'est-à-dire

$$100 \times \frac{\text{le diamètre bitemporal maximum en travers les orbites en centimètres}}{\text{le diamètre joignant les canthus internes de l'œil en centimètres}}$$

varie théoriquement entre 100 et l'infinité. Dans l'hypertélorism prononcé sa valeur est environ 200. Si l'on mesure sur des parties molles, l'index se trouve chez l'homme en moyenne entre 380 et 390. Mesuré sur des crânes l'index est un peu plus haut, c'est-à-dire 430. Comme l'hypertélorism semble être lié à la possession d'un gène dominant autosomique, il est possible que des valeurs extrêmes de l'index puissent servir de guide dans des affaires de paternité.

Zusammenfassung.

Der Interokular-biorbitalindex, welcher durch das Verhältnis

$$100 \times \frac{\text{maximaler, bitemporaler Diameter der Augenhöhlen in cm}}{\text{die inneren Augenwinkel verbindender Diameter in cm}}$$

dargestellt wird, variiert theoretisch zwischen 100 und dem Unendlichen. Bei ausgesprochenem Hypertelorismus beläuft sich der Wert auf ungefähr 200. Bei Homo liegt der Index bei Messung an Weichteilen im Durchschnitt zwischen 380 und 390. Kranial ist er etwas höher oder 430. Extreme Werte für den Abstand- und Spannweiteindex der Augen können möglicherweise in Vaterschaftsfragen als Anhaltspunkte dienen, da Hypertelorismus von einer dominanten, autosomalen Erbanlage abzuhängen scheint.

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LIBRI

M. Lamy: **Précis de Génétique Médicale,** Paris 1952, 256 pages, 108 fig. 38 tab.

French culture, it is sometimes said, pivots on the concept that human nature in all epochs and all cultures always has been, is and will be practically unchanged. In all men "*la raison humaine*" is assumed to be a single, indivisible, equal and identical entity. He who reasons thus finds comfort in Montaigne who wrote of truth and reason as the common property of all mankind. And similar ideas abound in the writings of Descartes and Rousseau, as well as in French classics generally. Obviously such a mental outlook is not exactly conducive to seeking out differences between human beings. Nor can Nazism and the German occupation have done other than curb the urge to differentiate in the sense laid down by the laws of genetics and make the French climate even more frustrating than before to the geneticist. Needless to say genetics is based on human beings having different traits and its objectives are to ascertain whether and to what extent these traits are hereditary or genetic. Be that as it may, few would quarrel with the statement that in France genetics has been a science in the doldrums.

In all fairness the recent publication in the French language of a textbook of genetics should consequently be classed as an event of major importance. Here the author, a paediatrician created professor of genetics in the Paris faculty of medicine, has given medical students a treatise on genetics which reads no less smoothly than it is instructive. The body of the book is of an elementary nature. Special chapters are devoted to dominant, recessive and sex-linked transmission, with apt examples of normal and pathological human traits. Sections of their own have also linkage and crossing-over, lethal genes, polyallelomorphism, polymerism and, though treated rather perfunctorily, population genetics and eugenics. Considering the author's clinical background, it is to be regretted that the chapter entitled "Panorama de la pathologie héréditaire" merely comprises a scant 20 pages. One would also have expected a paediatrician to have been awake to the importance of being able to tell the risk of being born with a hereditary disease. A wellcome addition esaily made to a new edition would be a few tables with risks expressed numerically. Otherwise the book contains numerous examples of clear and conveniently arranged tables well worth emulating. One would also have wished that Scandinavian clinical genetics had received fuller treatment. Some names mentioned in the text are missing in the bibliography, among them Cockayne which occurs in no less than four places—though Cockayne's large monograph on dermatological genetics, published 1933, is obsolete in some respects. Of Dalton, who was colour-blind and about whom Lamy relates the amusing incident that he was unable to make out the colours of his professorial robes, it is alleged that he was professor at Edinburgh. In the form of an appendix the author presents some common statistic methods, no doubt a very useful chapter for medical students who *par renommée* have neither practice with nor aptitude or inclination for figures. This is a valuable section for those who are stumped by texts in the English language (e.g. Fisher's, Dahlberg's, Stern's, etc.). It is to be hoped that this book will find readers far beyond the frontiers of France, for it is a good exponent of French scientific writing at its best.

T. Romanus, Uppsala

HEPATITIS

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Les Comptes Rendus contiennent in extenso tous les rapports et communications (environ 50) présentés au Congrès, ainsi que toutes les discussions dans le texte original.

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